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OF THE

ROYAL AERONAUTICAL SOCIETY

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Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

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NOTICES

Election of Members

The following members were elected at a meeting of the Council held on September 11th :—

Associate Fellows.—Captain A. B. Fanstone, A.F.C., E. C. Hubbard, J. D. Williams, F. T. Courtney.

Students.—R. Vasconcellos de Aboim, F. J. W. Digby, C. H. Pridham, W. T. Sandford.

Associate Members.—R. L. Preston, R. A. Tarleton.

Chairman

Mr. A. Ogilvie, C.B.E., Fellow, assumed office as Chairman of the Society on the first of this month. He has selected " Gliders and Light Planes " as the title of his inaugural address to be read at the Royal Society of Arts, John Street, Adelphi, on the 4th instant, at 5.30 p.m.

" Air Strategy "

It has been arranged with the Royal United Service Institution that Wing-Commander Edmonds's lecture on " Air Strategy " will take place before that Institution on Wednesday, December 12th, instead of before the members of this Society on Thursday, December 13th, as previously announced. Any members wishing to attend the lecture should apply either to the Secretary, or direct to the Secretary, Royal United Service Institution, Whitehall, S.W.1, for a ticket of admission.

Lantern Slides

The Council desire gratefully to acknowledge gifts of lantern slides to the Society's loan collection for the use of members by the following firms :—

The Supermarine Aviation Works Ltd.

Westland Aircraft Works (Branch of Petters Ltd.).

XUM

Library

The following books have been received and placed in the Library:—"Le Ballon, l'Avion," M. Larrouy; "Flying Round the World," W. T. Blake; "Regulations for R.A.F. Reserve," Air Council; "Sur la Théorie des Surfaces Portantes," M. Roy; "Engine Balance," Cormac; "Properties of Engineering Materials," Popplewell & Carrington; "Internal Combustion Engine," H. R. Ricardo; "Motor Fuels," E. H. Leslie; "Vom Gleitflug sum Segelflug," Gustav Lilienthal; "Proceedings of the Third Air Conference," Air Ministry; "Jane's All the World's Aircraft, 1923," C. G. Grey; "A Book about Aircraft," E. Protheroe; "Dictionary of Applied Physics—Aeronautics," edited by Sir Richard Glazebrook; "Jaarboekje of the K.N.V.L.," Besluit; "Anais do Club Militar Naval," Coutinho & Cabral; "Photography as a Scientific Implement"; "Inter-Imperial Communications Through Cable, Wireless and Air" (reprinted), Sir Charles Bright; Report of the Aeronautical Research Committee, 1923; "Taschenbuch fur Flugtechniker und Luftschiffer," 31st Edition, Moedebeck; "Etude sur le Ballon Captif et les Aeronefs Marins," Charles Lafon.

Council

In view of the Glider Competitions being held at Lympne by the Royal Aero Club from October 4th to 13th, the monthly meeting of the Council has been postponed to Tuesday, October 16th.

Arrangements for the Month

Thursday, October 4th, 5.30 p.m. Inaugural Meeting. Mr. A. Ogilvie, "Gliders and Light Planes."
Tuesday, ,, 16th, 5.30 p.m. Council Meeting.
Thursday, ,, 18th, 5.30 p.m. Squadron Leader R. M. Hill, "The Manœuvres of Inverted Flight."

W. LOCKWOOD MARSH, *Secretary.*

ROYAL AERONAUTICAL SOCIETY

A meeting of the Royal Aeronautical Society was held at the Royal Society of Arts, John Street, Adelphi, London, on Thursday, March 15th, 1923, Professor L. Bairstow in the chair.

The CHAIRMAN, in calling upon Professor Melvill Jones to read his Paper on "Control of Aeroplanes at Low Speeds," said that Professor Jones came before the meeting as a Fellow of the Royal Aeronautical Society primarily. He was also a member of the Aeronautical Research Committee, and was Chairman of the panel which dealt with control and stability. As a consequence of that, he was in a particularly good position to know how we stood in regard to that subject. The broad issue was very easily stated. It was within the knowledge of all that, when an aeroplane was stalled, control became exceedingly difficult, particularly lateral control; that was true of all heavier-than-air craft as we knew them at the moment. The point at issue, and the point which naturally came to the Aeronautical Research Committee for consideration was, "Must we always accept unsatisfactory control for stalling?" and he expected Professor Jones would deal with that point. He believed they would find his general outlook was that the problem was a very difficult one, but by no means hopeless, and that we might, in the course of a year or two, expect to so re-design the controls of an aircraft that the spin which was often precedent to a crash would be very much less common than to-day.

Professor MELVILL JONES then read his Paper.

CONTROL OF AEROPLANES AT LOW SPEEDS

I must begin by explaining how I come to be giving this lecture. The experimental work with which I shall deal has, for the most part, been done at the N.P.L. and the R.A.E., under the general direction of the Aeronautical Research Committee. The way in which I come to be connected with the work is that I am a member of this Committee and am Chairman of a small panel that was created, some three years ago, by the Committee, to deal with this and other work relating to control and stability. The experiments that I shall describe and the methods of dealing with the results that I shall employ are, therefore, the results of the combined work of a considerable number of people. I can thus claim no special ownership of any of the ideas that I shall use, except in so far as I belong to the panel that has been working upon them. On the other hand I am giving the lecture as a private person, so that any views I express are personal ones and in no sense official.

The policy of the A.R.C., in relation to the work under discussion, has been to issue experimental data as soon as it becomes available, but not to discuss methods of applying the data to practical problems until they are in a position to issue a comprehensive report on this aspect of the problem. The reason for adopting this course is to avoid annoying the public with half formed theories that may have to be withdrawn, whilst at the same time making the data available to anyone who is prepared to draw their own deductions from it. When, over a year ago, I agreed to give this lecture to-night, I hoped that some such comprehensive report, which I could have taken as my text, would have become available. The problem has, however, turned out to be very involved, and we are still no more than at the beginning, so that no such comprehensive report exists,

and I shall have to confine myself to a sort of interim report on progress, given with the principal object of inviting discussion and suggestions for future work. I shall to-night discuss only such data as has been issued, or is now sanctioned for publication, and I have to thank the Air Ministry and the separate authors for permission to use this latter data.

Safety in landing and getting off, which is the end towards which all this work is striving, depends, apart from engine reliability, upon two distinct factors :—

1. The minimum speed at which the aeroplane can fly steadily.
2. The control that the pilot has over its movements in the neighbourhood of this minimum speed.

The first factor depends on the shape of the wing and the loading per unit area; the decision as to what this minimum speed will be is a matter of the greatest importance with which I shall not deal to-night. I gave my views on this subject at the Air Conference, and I still adhere to them, in spite of the fact that they called down the wrath of the high gods upon my head. One must, however, deal with one thing at a time, and I shall, to-night, assume that the minimum speed has been settled, either with or without the use of variable wing sections, and that we are merely concerned to give the pilot the maximum power of control over his machine when flying near this speed.

I propose to start this problem from the very beginning, asking the indulgence of those of you who already know something about it.

When an aeroplane slows up from one steady speed to another, the angle of incidence of the wings must, in general, be increased to compensate for the loss of speed, otherwise the air lift will no longer equal the weight and the aeroplane will fall. There is a limit to this process, beyond which further decrease of speed will inevitably produce a fall. The angle at which this limit occurs is called the critical angle, or sometimes the stalling angle, and if it is desired to fly above this critical angle the speed must be again increased. The minimum speed of steady flight which is associated with the critical angle is called the stalling speed. Steady flight is quite possible either above or below the critical angle, but with the difference that, below the critical angle, the resistance to motion of modern aeroplanes is low and their controllability good, whilst above this angle the resistance is inevitably very high and, in modern aeroplanes, the pilot's control is almost non-existent.

When, to-night, I shall speak of flying beyond the stalling angle, or, briefly, "stalled," I shall not refer to flying at steady speeds lower than the stalling speed, which is impossible, but merely to the fact that the angle of incidence is greater than the critical and that we have therefore passed from the first régime above mentioned into the second. This is a point upon which confusion has occasionally occurred.

With this short introduction I wish to turn for a moment to consider the history of flying. All the pioneers of the art experienced immense trouble with their controls, and many of them were killed for this reason. Later on these control troubles seemed almost to disappear. Why was this? It certainly was not because much more had been found out about control; it was because, having better engines, men were able to fly faster and thus avoid approaching or passing the critical angle, near which the control difficulties that killed the pioneers arise. The difficulty had thus been sidestepped, not overcome, but it never could be sidestepped entirely, because of the necessity of landing and getting off. In a forced landing particularly, it is often a matter of vital necessity to approach the landing ground with the minimum possible speed, and it is not a matter for wonder that pilots, when faced with the necessity for doing this or having an obvious crash, occasionally overdo it and stall their aeroplane. When this

happens it is not unusual for control to be entirely lost and for a fatal accident to follow.

All down the progress of aeronautics there has been a steady series of fatal crashes due to this cause, and they still go on. In war we could afford to ignore these fatalities, but in peace we cannot, and I do not, myself, believe that there is any hope for a wide commercial development of aeronautics until this problem has been successfully tackled, so that the penalty of an error of judgment during the approach to a difficult landing is no longer more or less instant death.

How are fatal accidents from this cause to be reduced to a negligible quantity? In my own opinion it is no use whatever to take the line, "Pilots must not stall," "Engines must not fail." "Pilots should not stall," "Engines should not fail," are reasonable statements, but the fact remains that they do stall and fail respectively, and it is up to designers and scientists and such people to find out how the worst consequences of these errors can be avoided when they occur.

In my own view, and I think it is the view of the A.R.C., the only sound way to tackle this problem is to carry out a thorough investigation into the causes of loss of control, not only at angles of incidence at which the pilot may be expected to want to fly, that is at angles just below the critical, but at angles far above the critical which he may reach involuntarily. To me it seems unthinkable that we should leave uninvestigated a whole series of flying states into which it is possible to get an aeroplane through an error made in the ordinary course of flying; especially when the things that happen in these states are responsible for the majority of our flying accidents.

I lay stress on the words "thorough investigation." We are sometimes criticised on the lines that we are not getting along fast enough and that we ought to have tried this or that particular device that might have given us a controllable machine by now; in other words, that we fiddle about with experiments and theories instead of getting something done. Well, this brand of criticism leaves me, personally, quite unmoved. We are not aiming at short cuts to a success that we do not understand. What we want is to understand the problem thoroughly, so that we may be able to develop some broad basis upon which aeroplanes, under design, can be examined for controllability, much in the same way as they can now be examined for strength and performance. We have, as a matter of fact, made one or two freak alterations to aeroplanes and tried them in flight, but this was done more from the point of view of amassing information on which we could check theories and wind channel data than with the object of evolving the perfectly controllable aeroplane at this stage of the investigation.

Let me now define what I mean by the word control. Of course, no aeroplane is infinitely controllable, there are always limits to the rates at which they can be moved and rotated. A stalled aeroplane without engine, for instance, must descend at a rather high speed whatever else it may be doing, so that, in one sense, it is bound to be less under control than an unstalled aeroplane in which the inevitable rate of descent is much less. This is an inherent difference between the two states, and we cannot overcome it. We can, however, by altering the form of the controls, give the pilot more or less power over the orientation of his aeroplane, and in future, when I speak of control, I shall limit my meaning to control over the orientation of the aeroplane.

It is quite easy to give, in general terms, an explanation of the reasons for loss of control when stalled. When an aeroplane is rolling so that the starboard wing tip is falling, the local angle of incidence of that tip is increased, whilst that on the other tip is decreased. Below the critical angle increase of incidence increases the air reaction on the wing, hence in this case a rate of roll

to starboard results in a rolling moment tending to raise the starboard wing and thus to stop the roll. When flying above the critical angle, however, increase of angle of incidence decreases the air reaction, and a rate of roll to starboard generates a rolling moment to starboard, and hence the rate of roll tends to increase unless checked.

This phenomenon is called autorotation, and has been known for years; it can easily be observed by pivoting a model aeroplane in a wind channel about an axis parallel to the wind. Provided this model is stalled, a roll, once started, will continue indefinitely.

The above is not the only effect of rolling a stalled aeroplane, the downward velocity imparted, say, to the starboard tips not only reduces the reaction upon it, but increases the aerial drag upon it, the reverse being the case on the rising tip. In this way a yawing moment forcing back the falling wing tip is introduced. Now this yawing moment, unless checked, quickly starts the aeroplane turning to starboard. This speeds up the port wing relative to the starboard wing, and thus, by increasing the air reaction on the port wing and reducing that on the starboard wing, introduces an additional rolling moment tending still further to increase the rate of roll to starboard.

If these tendencies are not checked it is easy to see how the effects pile up rapidly and produce the familiar spin. Now why cannot the pilot check the tendencies as soon as they start? It is not that they are too rapid; they are rather rapid in developing, but the average pilot is more rapid. It is not, as we thought at first, that the ailerons will not give the necessary rolling control to neutralise and reverse the effect of the rate of roll. The reason is as follows:— When the ailerons are used to check an undesirable rate of roll they generate a yawing moment of the same sign as that which is already being generated by the rate of roll, and the two combined will, as a rule, overpower any rudder of conventional size that is supplied with modern aeroplanes. Although, therefore, the direct action of the ailerons is of the kind desired, their indirect action upon the rate of yaw undoes all the good they have done, and the last state of that aeroplane is worse than the first.

Two obvious cures for this ill present themselves. One is to provide such a big rudder that the pilot can hold up against any yawing moment that may occur, and thus use his ailerons with impunity; and the other is to provide freak ailerons that will give the opposite yawing moment to that given by the conventional type, for if this could be done a simple movement of the ailerons would neutralise both the effects of rolling a stalled machine and leave very little to be done by the rudder.

Another cause that contributes to the loss of control after stalling is that the centre of pressure of the wings moves backwards as the critical angle is passed, so that the aeroplane tries to dive downwards. In many aeroplanes, especially those that are stable in normal flight, the elevators are not powerful enough to counteract this tendency, and an uncontrolled dive results. To cure this trouble it is obviously necessary to make the longitudinal control system more powerful. This may not necessarily mean larger controls, but merely more movement in the trimming arrangements for the fixed part of the tail. This question has not yet been fully investigated.

Here then you have, in a nutshell, the whole situation, together with the directions in which a cure is to be sought, but do not be misled into the belief that because we can formulate the problem clearly in this way the trouble is over. It is when we come to consider quantities and ways and means that the troubles really begin. When we came to tackle the quantitative side of this problem, some three years ago, we were met with a complete lack of data, for the reason that almost all wind channel experiments on controls had hitherto been confined

to angles below the critical, and nearly all free flight experiments had been confined to flying below this angle and to spins. Spins are the last stages of lack of control, and therefore are not very interesting from our point of view, which is to find out how to prevent the loss of control occurring in the early stages. If I may use a simile from mountaineering, the rock climber is more interested in finding out how to avoid slipping from the rocks than in studying the precise nature of the bump with which he will hit the ground far below.

May I digress a moment on to the vexed question of the use of models, for the situation in which we found ourselves bears strongly upon this problem. It is not until you have got no model data and, therefore, as things are, no detailed data at all, that you realise how difficult it is to think quantitatively when you have no quantities to think about. In this case we knew that we were working in the region, above all others, in which models are likely to give misleading results, but the alternative to using them was to sit and think about nothing. The plan that we have adopted to get over this difficulty is to use models to give us something to think about, trusting that the general character of the data will be correct, but firmly refusing to trust any conclusions until they have been thoroughly checked on the full scale.

When one comes to deal with these problems of control from a quantitative point of view, one is immediately rebuffed by the appalling complication involved in any attempt to calculate the motion of the aeroplane under the action of the controls. The classical stability calculations of Bryan and others are complicated enough, although they only deal with infinitesimal disturbances from steady straight flight. If we were to attempt to attack our problem in the same way, we should be involved in hundreds of times this complication before we could make a complete statement of the more involved motions.

From the practical point of view, however, it is not, in general, necessary to determine exactly how the aeroplane will behave under some fixed setting of the controls, it is usually sufficient, and indeed more interesting, to know whether the pilot has the power to apply any turning moment that he may desire to the aeroplane, when the latter is moving in any particular way. All experience goes to show that if a pilot *can* influence the aeroplane in any way he likes he may safely be left to find out *how* to do it for himself. There appears to be no need to carry out elaborate calculations in order to tell him what to do.

Acting on these principles we attack the problem quantitatively by trying to find out under what conditions the pilot will be able to apply moments in any way that he may desire, and under what conditions he will not be able to do so. There is a very simple way of distinguishing broadly between these two sets of conditions. If a pilot can produce, about any particular axis, a moment in either direction, he can also produce zero moment about that axis. It follows from this that in all those cases where he can produce any moment he likes about any axis he likes he can also bring the aeroplane into a state of complete balance, such that no moments are acting about any axis whatever passing through the C.G.

The power to produce a state of complete balance under any particular set of conditions is, therefore, a first necessity of control, although the possession of such a power does not necessarily imply that there is sufficient control for practical purposes, since moments of some definite magnitude will be required to provide the necessary angular accelerations involved in the manœuvre in question. We may therefore say that the power to produce balance is a *necessary* but not *sufficient* criterion of controllability.

The plan that we have adopted to study control quantitatively is to concentrate attention first upon finding out whether it is, or is not, possible to reach a state of balance under any set of conditions. If it is not, then the aeroplane

is obviously not under control under those conditions; if it is possible, then the aeroplane may or may not be under *adequate* control, depending on the amount of free control moment that can be applied after balance has been reached.

As an example, we have already seen roughly why an aeroplane becomes uncontrollable when stalled, and we observed that the reason was that under certain conditions (*i.e.*, when rolling more than a certain rate) it was no longer possible to maintain balance, owing to insufficient rudder power.

Of course the consequences of not being able to reach the balanced state will depend a good deal upon whether the moment that we cannot neutralise tends to make things worse or better. Thus we cannot roll an unstalled aeroplane at more than a certain rate, but if it does roll more rapidly the unbalanced moment tends to stop the roll and, unless the roll is necessary to the manœuvre, no harm is done. In the stalled state the reverse is the case, and the lack of control has very serious consequences.

Let us bear these facts in mind for future reference and proceed to the study of the data available.

The main variables concerned are as follows:—The angular settings of the three controls, the angle of incidence, the rates of roll, pitch and yaw, and the rate of side-slip. A complete investigation would involve a determination of the three forces and three moments that act on the aeroplane for every combination of these eight variables. Such a complete experimental investigation is, of course, out of the question and some simplification is essential. The first simplification that we make is to neglect the effect of rate of pitch on rolling and yawing moment, and of rate of roll and sideslip on pitching moment. It is not rigidly correct to do this, and we can see cases in which we shall have to take such effects into consideration; still, we must begin by simplifying somewhere, and this seems, on the whole, the best way to start.

Let us deal with what are called the asymmetric or lateral group, that is with roll yaw and side-slip. For a complete investigation, even of this restricted group, we would have to find the effect upon the air reactions of every combination of ailerons and rudder setting, at every combination of rates of roll, yaw and side-slip, with the aeroplane flying at every angle of incidence. This is still too big a job to be practical, even if we could do it experimentally, so we make the further simplification that we will only study the effects of pure rates of roll, yaw and side-slip; each in the absence of the others.

We have no guarantee that the effects of a combined yaw and roll can be obtained from the addition of the effects of each taken separately, but there are reasonable grounds for supposing that two aeroplanes that behave similarly when subject to pure rates of roll, yaw and side-slip, will also behave similarly when subject to combined movements. If this should prove to be the case we could get all we want in practice by a study of the simplified data for pure rolls, etc.; for what we generally require is to be able to predict whether some particular aeroplane, under design, will be better or worse than some other one of a different shape, upon the control of which we have experience in actual flight.

Now how can we get at the effects of pure rates of roll, yaw and side-slip upon the air reactions. Well, we can make a shot at them by calculation, based on strip theories similar to those used in the design of airscrews. But if we are prepared to use models we can also determine them experimentally.

The effect of side-slip can be determined by merely turning the model in the wind channel until the wind blows on it from the side, and then measuring all the forces upon it; the measurements being repeated, of course, for various angles of incidence and for various amounts of side-slip. Apparatus for doing this has been designed at the National Physical Laboratory, and is used in one of their 7ft. channels.

The effect of a steady rate of roll can be examined by means of a special apparatus designed by the Royal Aircraft Establishment and used in one of their 7ft. channels. In this instrument the model is rotated steadily about an axis parallel to the direction of the wind. Such rotation does not alter the attitude of the model to the oncoming air, so that steady forces and moments are set up that can be measured.

The effect of a rate of yaw cannot be studied in the wind channel, but could be studied on a whirling arm, such as is used for testing airscrews. These experiments have not yet been done, but it is hoped to revive the small whirling arm at the N.P.L., for this and other reasons. For the purposes of this lecture I have, however, made a rough calculation of the effects of yaw, and, though this is based on admittedly shaky assumptions, it probably gives results of the right order, sufficiently accurate for the somewhat rough arguments that I shall be using.

Fig. 1 shows some typical data of the type we have discussed. The figures

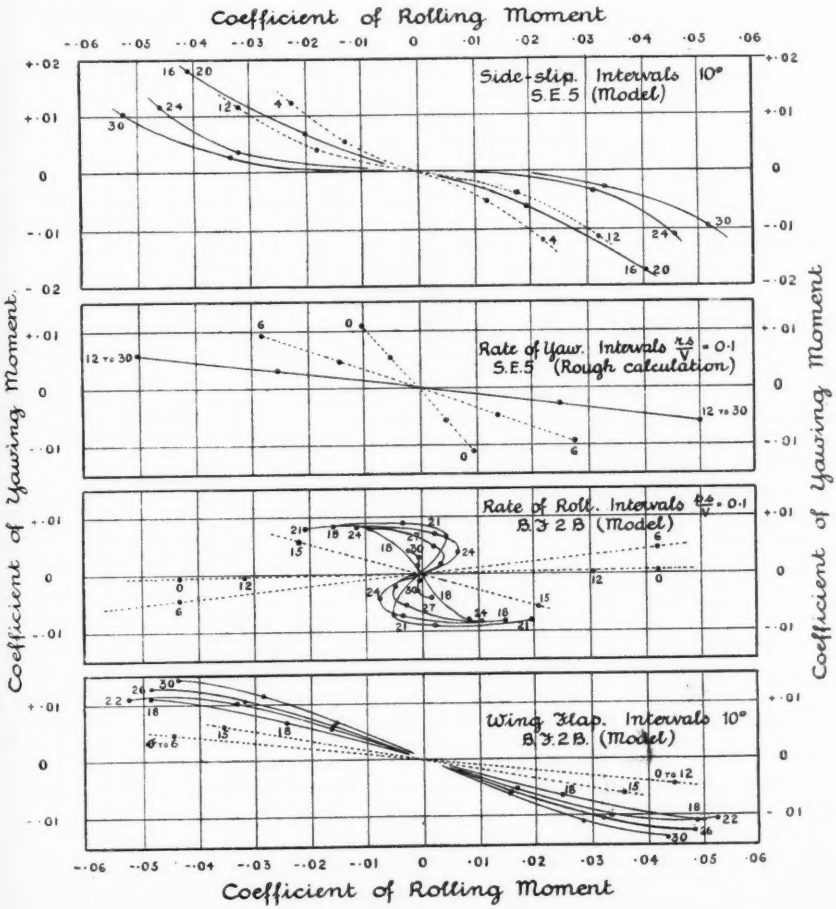

FIG. 1.

refer to the rolling and yawing moments produced by various rates of side-slip, yaw and roll and by the ailerons in straight steady flight. Rolling moments are plotted as abscissæ and yawing moments as ordinates to the same scale, but it should be noted that the abscissæ represent rolling moment, not about the direction of motion, but about an axis in the body inclined at about an angle of — 3 deg. to the chord of the wings. It will be remembered that the rates of roll and yaw were measured about axes that were parallel and perpendicular to the direction of motion. The reason for adopting this somewhat peculiar procedure is that it very greatly simplifies the representation of the results and the physical conceptions involved. It will be noticed that the line joining the origin to any point on the diagram is a vector representing in magnitude and direction the resultant moment acting on the aeroplane.

The units in which these results are represented are of a non-dimensional nature similar to that used for representing lift and drag. They may be considered to have been obtained by dividing the actual measured moment by the quantity $\rho S^2 V s$

where ρ is the air density
S is the area of the plane
V is the air speed
and s is the semi-span.

For those who are not used to these non-dimensional systems the figures can be considered as being proportional, for a given aeroplane at a given height, to the actual moment divided by the square of the air speed.

The separate curves in the figures represent results for different angles of incidence of the centre part of the wing, those for the unstalled régime being shown dotted and for the stalled régime in full.

Let us take the figures in order. The consequative dots on the first group of curves represent the effects of side-slipping 10 and 20 deg. respectively. It will be observed that side-slipping produces both yawing and rolling moments, *i.e.*, a positive side-slip, to starboard, produces a negative rolling moment, lifting the starboard wing, and a positive yawing moment, turning the nose to starboard. The effect of side-slipping increases as the angle of incidence increases until about 24 degrees incidence, when there is a rapid fall in the yawing moment, except at high angles of yaw. This is no doubt due to the body shielding the fin and rudder at these high incidences.

The next diagram, showing effect of rate of yaw, is only constructed from very rough calculations. It shows that, after stalling, the effect of rate of yaw is nearly independent of angle of incidence, and that the rolling moments generated are much greater than the yawing moments. The consequative dots in this and the next diagram represent rates of rotation, such that the velocity of the wing tip due to the rotation alone is 0.1, 0.2, 0.3, etc., of the speed of the aeroplane.

The diagram showing the effect of a rate of roll about the direction of motion is the most interesting of the lot. It shows that, below the stall, the effect of rolling is to introduce an immense rolling moment tending to stop the roll, whereas above the stall the result is a relatively small rolling moment, in the direction to increase the roll, accompanied by a yawing moment retarding the falling wing. This is, of course, the phenomenon that we have already discussed qualitatively. It will be noticed that the effect practically ceases at 30 degrees incidence, where rolling produces practically no moments at all. The dots on the curves have the same meaning as in the previous diagram.

Turning now to the action of the ailerons in straight flight, we see that the effect of these is to produce rolling and yawing moments of opposite sign, as already explained; but we see now that, after the stall, the desirable

rolling moment is much decreased, whilst the undesirable yawing moment is increased.

If we assume that the effects of the ailerons are the same whether one is rolling or not, we can see, from the last two diagrams, that it is impossible to neutralise the effects of a rate of roll of, say, 0.1 at 24 degrees incidence with the ailerons alone; for, if we used them to destroy the rolling moment, we should be left with a large unbalanced yawing moment, and an inspection of the diagram shows that this unbalanced moment would amount to about 0.008. The Bristol Fighter rudder at this incidence only provides about 0.003 yawing moment; hence we see where the difficulty lies.

One cannot, however, assume lightly that the ailerons will behave in the same way when flying straight as when rotating in roll or yaw, but there is no difficulty in extending the experiments to find out how they act when any of these simple motions are occurring. These experiments have been made in the case of side-slip and rate of roll, and they lead, of course, to a whole series of observations at different aileron settings for each spot upon the three upper diagrams of Fig. 1. It would be useless to represent here every one of these experiments, they can be found in the official reports. I shall content myself with using one particular instance to explain how we deal with this data to find out the conditions that will bring about a state of balance in any particular case.

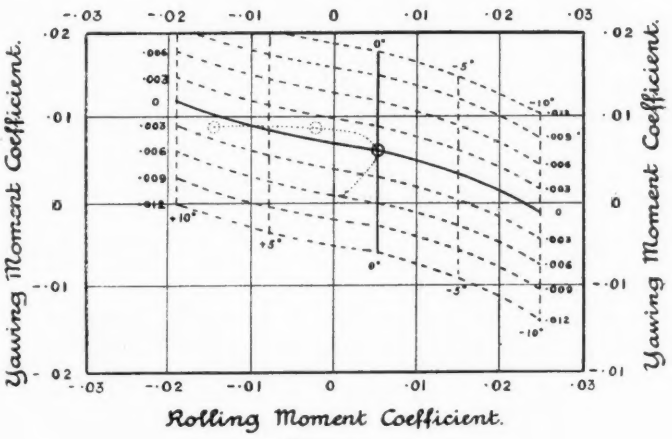

FIG. 2.

Fig. 2 represents the effect of the controls when the angle of incidence is 21 degrees and the rate of roll 0.10. The rectangular co-ordinates represent moments, as in Fig. 1, and the thick oblique line represents the effect of the ailerons under these conditions. The symmetrical position of the ailerons leads, in this case, to definite yawing and rolling moments owing to the presence of the rate of roll. The rudder applies a pure yawing moment; hence the effect of moving the rudder is represented by vertical movements in the diagram. It will be apparent that the settings of the ailerons and rudder that are required to bring about any particular moment upon the aeroplane can be instantly read off upon the distorted co-ordinates—shown dotted. We can see, for example, that to bring about

a state of balance, or zero moment, it is necessary to set the ailerons at 2 degrees and for the rudder to provide a moment of 0.007.

When we have treated all the information in this way and extracted the aileron and rudder settings necessary to bring about balance in each case, we get a mass of data that is represented in the six diagrams in Fig. 3. These

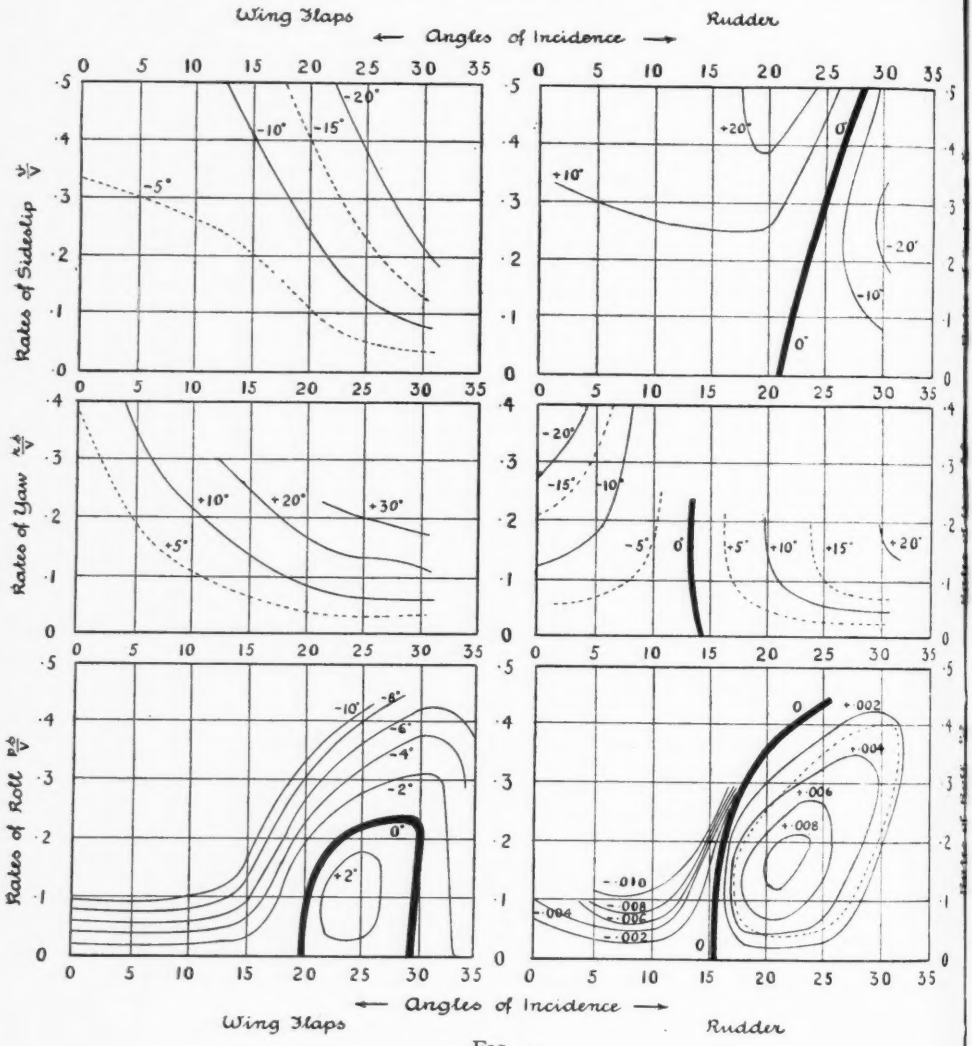

FIG. 3.

show the rates of roll, yaw and side-slip at which balance can be obtained with given settings of the controls. For example, when side-slipping at 0.25 of the forward speed, with incidence 20 degrees, balance could be obtained with — 10 degrees aileron and + 10 degrees rudder.

It is these curves that we hope will be characteristic of the controllability of the aeroplane; that is to say, we hope that aeroplanes for which these six curves are similar will in practice behave similarly. It remains for experience to decide whether this will be the case.

The curves of this diagram do not represent any one particular aeroplane. The side-slip curves are from a model S.E.5, the roll curves from a model Bristol Fighter, and the yaw curves from rough calculations of my own, which may be taken roughly to represent the S.E.5. The curves should, however, be sufficiently typical of most modern aeroplanes to allow of our discussing broadly their general features.

Let us see what information we could obtain from these curves concerning any aeroplane of which they are representative. Positive control angles mean stick to the left and left rudder. Positive side-slip is to starboard. Imagine a side-slip to starboard of 0.2 times the forward speed. The curves show that, in the unstalled régime, this requires right stick and left rudder of quite reasonable amounts, but that, as the angle of incidence increases, the necessary aileron angle increases rapidly whilst the rudder changes sign.

Now for the effect of rate of yaw. A positive rate of yaw means turning to starboard. A flat turn to starboard without side-slip, if such a thing can be imagined, involves, in the unstalled state, left stick and right rudder, and not much of either. After stalling, the amount of stick moment necessary is greatly increased and the rudder crosses over against the turn, very large angles being needed to get any rapid turn at the higher incidences. The reason for this apparent paradox is easy to understand. The effect of the turn to starboard is to raise the port wing, and to counteract this, such large aileron angles are required that they turn the aeroplane by their secondary effect on yawing moment, and this effect has to be counteracted by the rudder.

Now consider the effect of rate of roll. A pure rate of roll to starboard requires right stick and rudder in the unstalled state, and large control moments are required to get rapid rolls. After stalling, however, left stick and rudder are necessary; that is, the roll must be prevented from increasing. This, as we have seen already, is one of the primary reasons for the difficulty of stalled flying.

It will be noticed that, in the rudder-roll diagram, angles are not given, but instead the moments required from the rudder. It is convenient to do this because in this case the action of the rudder is not seriously affected by the rate of roll, so that the diagram can be applied to any rudder of known power. The peculiar interest in this diagram lies in the fact that there is one particular spot on it at about 22 degrees incidence and 0.18 rate of roll where the rudder moment required for balance has a maximum value. If the rudder power exceeds this value it will be possible to keep the aeroplane in balance over a very wide range of rates of roll and angles of incidence; but, if it falls much short of this value, quite a small rate of roll will send the aeroplane out of control.

The power of the Bristol Fighter rudder, for instance, at about 22 degrees incidence is about 0.003 on this scale, so that, if its controllability is represented by these diagrams, it should go out of control, when stalled, at a rate of roll of only 0.025 or thereabouts. This works out to about 60 degrees in 12 seconds, which is quite a slow motion.

Imagine an aeroplane that has a maximum rudder power just equal to the maximum rudder power here required. Under the worst conditions it will just be possible to reach a state of balance when a pure rate of roll is taking place, but there will be no margin over, neither to maintain balance when side-slips and rates of yaw are superimposed, nor to provide angular accelerations. Such a machine, though not hopelessly uncontrollable, will not be under adequate control.

The suggestion now made is that the reserve yawing moment available, over

and above that required to cope with a pure roll under the worst conditions, should be considered as a criterion of the controllability of the aeroplane when stalled. A single number of this nature cannot, of course, be a sufficient and only criterion of controllability. The six diagrams of Fig. 3, at the very least, will be necessary to specify controllability in detail, and we have said nothing about the stability of the balanced states, which will certainly have an important influence. The contention that I now make is, however, that this number will, more than any other single number, be representative of controllability in the stalled state.

Let us see how this criterion can be applied to existing data. The following figures are taken from wind channel experiments:—

Aeroplane.	Approximate value of the criterion.
Bristol Fighter	— 0.005
Standard Avro	0.000
Avro Large Rudder	+ 0.010

In practice we know that the Bristol Fighter is notoriously bad on the rudder at slow speeds, although it can, with great care, be flown stalled. I am unable at present to say definitely whether it is, or is not, possible to stop the initial stages of a spin with the rudder in this machine, but I think not. The standard Avro can just be controlled with full rudder at any stage in the development of a spin, whilst with the big rudder there appears to be ample rudder power under all conditions.

So far, therefore, the simple criterion suggested appears to represent the known facts approximately, but the full scale data is not yet nearly complete enough to make any definite statement on this point. The pilots are quite clear that the control of the Avro, even with the big rudder, is not as good as they would like, although they do not complain of lack of rudder power, as they do with all other stalled aeroplanes. It would appear, therefore, that other considerations, besides this simple criterion, must be taken into account in specifying an aeroplane that shall be adequately controllable when stalled, but it remains for further experience to show what these will be.

I have not discussed the question of the stability of these balanced states because we do not understand it yet. It must not be inferred that differences of stability will have as little effect on problems of control as they do in the unstalled state. In unstalled flying the types of instability that occur in conventional aeroplanes develop so slowly that they are checked by the pilot with the greatest ease. In the stalled state, however, they develop much more rapidly, and although they are still not too fast to be checked in time by an alert pilot, they develop fast enough to affect the ease of control quite appreciably.

That is about as far as we have got at present. Let us now consider future work. The pilots of the R.A.F. have developed a whole art of stalled flying, being able to fly steadily enough to take measurements up to angles of incidence of 40 degrees, but they have never yet had a machine on which they can do this comfortably, owing to the fact that we have not yet been able to alter an Avro so as to have big enough aileron and tail moments. This is being done, but the process of altering standard aeroplanes in the radical manner required is laborious and unsatisfactory, and we hope shortly to have a special type of aeroplane designed in which all the alterations that are required for research work on this subject can be easily made. If and when we get this machine, progress should go on with a jerk, provided that economy has left anyone to carry on the experiments.

The direction in which we hope to proceed is, first, to get an aeroplane that can be satisfactorily controlled when stalled—we have not quite done this yet—and, next, to get this effect with the least ugliness in design, that is to say, with reasonable sized rudders, etc. We hope to do this by finding forms of

aileron that will give the necessary rolling moment combined with yawing moments of the opposite sign to those given by ailerons now in general use. There seems to be every hope of doing this by using Handley Page slots on the wing tips, in conjunction with the ordinary ailerons; we are experimenting on this problem at the moment.

The prospect of eliminating the particularly fatal kind of accident that follows an involuntary stall near the ground in present-day aeroplanes is, in itself, sufficient inducement to carry on this work as vigorously as possible, but we have a faint hope that we may be able to go even further than this and deliberately use the stalled state to assist in making difficult landings. One of the principal difficulties of landing an aeroplane in the ordinary way is that it is impossible to alter the angle of glide without stalling on the one hand, or gathering speed on the other. The pilot who finds himself too high has therefore to resort to some terrifying stunts in order to lose height, and terrifying stunts at low speeds near the ground are not pleasant things. The peculiarity of stalled flying, on the other hand, is that you can come down at any angle you like without appreciably altering speed, and this is exactly what you want when landing.

We are still a long way off being able deliberately to use stalled flying to assist a landing, but some of us have hopes of getting there in time, and if we can do so, I, personally, believe that it will work a revolution in the safety of flying.

Discussion

Colonel Ogilvie, after thanking Professor Jones for his interesting lecture, said he had one small criticism to make. Professor Jones had said that in the early days the number of people who killed themselves became less as the power of the engines became greater. He himself believed it was the other way about. It was when people began to fly easily that the caution exercised by the earlier people was neglected, and the number of accidents increased. He should think that one of the main reasons why stalling decreased in this country was the introduction of means of seeing how fast one was going.

Another point he would like to put to Professor Jones was that, when thinking of this problem of stalling, why were we always so interested in trying to raise the lower wing? Having got that wing down and on the point of stalling, why should we not pull the other wing down to it? There might be a line of thought in that direction which would prove useful.

Another matter which he would like to bring to the attention of the meeting and one which was much more important, was that there was a movement among some of the rather influential circles in this country to give up endeavours towards safety and push towards performance. That was really a serious danger. There had been several indications of it, and unless we in the Society stood out against it and prevented it happening we should be worse off than we are now.

Mr. Handley Page said that it seemed to him that sufficient emphasis had not been laid on the fact that the stalling point should be delayed as far as possible. It was granted by everybody that the critical period of control was that which occurred round about the stalling point, and therefore the longer this was delayed the easier became the problem of control. In his opinion it was much better to fly unstalled at a large angle of incidence than to have to adopt special means for control under stalling conditions.

Both of these questions, namely, being able to fly unstalled up to, say, 45 deg., or flying steadily in the stalled condition at the same amount, required a great deal more research, and he looked forward with great interest to the further experiments which Professor Jones had outlined, and the combination of the use of the slot in front of the wings combined with the aileron.

He hoped that the research that was necessary to elucidate a good many difficult problems would be continued.

Squadron Leader R. M. HILL said he felt that, after what Professor Jones had said, there was little he could add. In any case, this subject was a very difficult one to discuss at the present stage, and, considering the difficulty, he thought that Professor Jones had dealt with it in a wonderful way, inasmuch as he had presented a vivid picture of the experiments carried out, of the difficulties surrounding them, and of what results they might be expected to yield.

What was the commonest and most serious kind of flying accident? It was the class of accident ensuing on an involuntary stall. However they might blink the fact, what had hindered flying from becoming popular in the sense that people were not apprehensive of it, was that an aeroplane, when it got into the stalled state, became temporarily uncontrollable. It was not as if this were a rare or unlikely occurrence. There was, indeed, a variety of reasons that led to involuntary stalling, the results of which were so frequently fatal. It was useless, as Profesor Jones had pointed out, to attempt arbitrarily to prevent a pilot stalling; the alternative was therefore to study the phenomenon of stalling and find means of mitigating its effects. This would become possible only when the pilot was given the power to exert control over the aeroplane, not only just below the stalling incidence, but well above it. The changes in an aeroplane's behaviour at stalling must be made less critical if the aeroplane was to be maintained in a state of steady flight at and above its stalling incidence.

How could the problem be solved? Professor Jones had suggested lines of thought which it was hoped would lead to a solution. From the flying point of view this solution would mean that the pilot could go up in his aeroplane, and, if the engine failed near the ground, he could pull back the control stick and keep it back without being deprived of the power of orienting the aeroplane at will. The worst that could then happen would be that it would strike the ground at a downward velocity of not more than 25ft. per second, a forward speed little in excess of its stalling speed, and with the fuselage practically horizontal. That was the sort of crash that might shake, but it would not kill. If it were necessary to make a pointed comparison, a description of the sequence of events which led up to the present day crash consequent on stalling would serve; they were, however, only too well known. Squadron Leader Hill added that if the results hoped for were achieved, and the experiments so carefully tended by Professor Jones bore fruit, flying would be revolutionised.

Professor MELVILL JONES, in reply, said: Squadron Leader Hill's speech puts the case for continued work on the problems associated with the control of a stalled aeroplane very clearly; it requires no answer from me, and I will merely state that I am in agreement with everything that he has said.

In answer to the points made by Colonel Ogilvie. When I implied that a higher proportion of the early pilots met with disaster than is the case with modern pilots, I had in mind the very early pioneers in the period before and just after the Wrights appeared on the scene. I do not think that there is any doubt that these people did experience great trouble with their controls and that many of them were killed in consequence. It was, in fact, the great skill shown by the Wrights in grappling with these difficulties of control that differentiated their work from that of their predecessors. With regard to the idea of lowering the upper wing rather than attempting to raise the lower one, both methods of tackling the problem are being investigated experimentally.

I was glad to hear Colonel Ogilvie raise the question of the relative importance of safety and performance. I am strongly of the opinion that provided a cruising speed of about 100 miles per hour can be obtained it is infinitely more important, in the commercial sense, to reduce all dangers of a fatal crash to a vanishing point than to obtain still greater speeds, and I am in direct disagreement with the

opinion, expressed freely at the last Air Conference, that it will be practicable to attain this end by the elimination of engine failure. By all means let us reduce engine failures to the lowest possible limit, but, on top of that, let us arrange, if we can, that a failure when it does occur seldom if ever results in a fatal accident. Both these achievements are in my opinion necessary before the ordinary man going about his ordinary business will sit up and take notice of aerial transport.

In answer to Mr. Handley Page, I would say that there are two distinct problems to be solved—they are to fly slowly and to have control when doing so. I stated at the beginning of my paper that I would confine myself to the latter. This does not mean that I am not awake to the importance of obtaining the slow speed, whether by means of low loading or by the use of high-lift coefficients. It merely means that the two subjects, taken together, are too big to be dealt with in one lecture. Possibly Mr. Handley Page considers that if the stall can be postponed to a large angle, the necessity for studying the control after stalling will not arise; if this is his view, I differ from it, because there is no limit to the slowness with which a pilot would like to fly if he could do so safely; the result of putting off the stall and thus increasing the lift coefficient will, therefore, almost certainly be that the aeroplane will be treated as having a lower landing speed than would otherwise be the case and there will be the same danger of overstepping the safe condition as at present. Our object up to the present has been to reach an understanding of the problems of control after stalling, and we have considered that this is more easily done on conventional aeroplanes, to which we are all accustomed, than by mixing the two problems of controlling an aeroplane and of obtaining specially high-lift coefficients. There is, however, no doubt that the two problems will eventually have to be studied together.

A hearty vote of thanks to Professor Melvill Jones for his exceedingly interesting and inspiring lecture concluded the proceedings.

Contributed Notes on Prof. Melvill Jones' Paper by Major T. M. Barlow.

I should like at first to congratulate Professor Jones on the definite stand he has taken up against those short-sighted critics who wish to push along with such complicated research problems as outlined in this paper, on the "hit or miss" principle. From my own experience in the testing of new types it is quite evident that as yet we are only on the fringe of definite information as to the most efficient (in all senses of the term) form of controls. At Martlesham Heath we have for some time now, as a matter of routine, carried out slowest speed trials, using the Camera Obscura method, and these tests have emphasised the difficulties which designers, through lack of information, have had to contend with to produce adequate control throughout the large speed ranges now ruling in high-powered aircraft. I hope, therefore, the work initiated by Professor Jones will be extended to controls over the whole speed range.

At Martlesham also we have been obtaining records of longitudinal stability of most aircraft from slow to top speeds by the Pitching Camera. These all provide a certain amount of useful data. It seems to me, however, that it should be possible for the Advisory Committee Panel to draft out some form of flight tests which, with suitable apparatus, could be carried out on all types, and thus a collection of reliable technical data would be obtained. I am in agreement that a special machine would be of immense help to deal with the more intricate problems, but progress will undoubtedly be hastened if information could be collected systematically from firms' tests pilots and Service experimental stations. There would be difficulties probably in the matter of suitable instruments and apparatus, but not insurmountable.

Stalled flying with full control will have to come, and this I think will be attained by following to a certain extent nature and using in flight variable areas and cambers with perhaps special devices such as the Handley Page slotted wing.

ON THE VORTEX PAIR QUICKLY FORMED BY SOME AEROFOILS

BY N. A. V. PIERCY, D.SC.

It was suggested by Lanchester* some years ago that an aerofoil immersed in a moving fluid gave rise to two trailing vortices of opposite hand, one situated near each of the extremities of span. The existence of the vortex pair has since been demonstrated in a variety of ways, of which perhaps the most striking is that due to Caldwell and Fales,† who used the condensation of water vapour in a high-speed wind channel to obtain photographs. The vortex pair constitutes so important a feature in the stream behind most aerofoils that measurements capable of numerical analysis seemed desirable, and an account is given in the present Paper of some work to this end which appears capable of straightforward interpretation.

It may be that the residual motion sufficiently far behind any aerofoil is a vortex pair. Similar experiments to those about to be described, however, which deal with the motion only for a few chords back, have been made with several other aerofoils of a variety of shapes having long, graded wing tips, and with some of these no distinct vortex was found. With others apparently more than one pair could exist in certain circumstances, but opportunity has not occurred to probe deeply into these interesting cases. Attention is confined to two aerofoils representative of considerable variation in a class having uniform section and angle of incidence with blunt, or square-ended, wing tips. The general nature of the results is common even with aerofoils outside this class, but the foregoing remarks will serve to indicate that exceptions are known to exist.

In the course of the investigation an attempt is made to throw some light on the more general question of the structure of a vortex well on into turbulent flow. If the eddies are to be usefully employed in aerofoil theory—or indeed, since they are so commonly to be found in real flow past other bodies, in any synthetic account of a fluid motion problem which avoids a solution of the viscous equations—information on this score is hardly less important than knowledge relating to position and strength.

The experiments were carried out in the 4ft. wind channel of the aeronautical laboratory of East London College. The channel was driven from the battery of the electrical engineering side (by kind permission of Prof. Macgregor-Morris), so that the velocity was kept exceptionally constant. The apparatus for measuring the angle of the stream was as follows. The angle head was of similar form to that for some time used at the N.P.L., and consisted of two fine tubes, inclined at 45 deg. to the stream, with their open mouths touching (see sketch in Fig. 1). This was mounted on a cranked arm turned by a micrometer wheel outside the channel, so that it could be rotated without displacement of the mouths of the tubes. The turning gear was mounted on the tail-stock of the lathe, which in turn was carried on the lathe bed—fixed outside the channel at right-angles to the wall. Thus traversing of the instrument along a line parallel to the trailing edge of the aerofoil was arranged for, as well as orientation about that line as axis. The aerofoil could be traversed without change of incidence in the direction of its lift. The two tubes were

* Lanchester, "Aerodynamics," page 171 (1909 edition).

† Caldwell and Fales, American Advisory Committee for Aeronautics, Report 83 (1920).

connected across a Chattock gauge and, in use, were oriented until the pressures in them were equal. Errors in the manufacture of the instrument, warp of the lathe bed, and angle variations in the channel not due to the model were allowed for by a preliminary exploration with the model removed. Speed variation with position, not caused by the aerofoil, was also determined in a similar manner.

Character and Strength of the Vortex

Aerofoil *A* was rectangular in plan-form, 24in. span by 4in. chord, with a thick section somewhat resembling that known as R.A.F.19. At 8deg. incidence and a speed of 31.3ft./sec., the distribution of downwash angle along a line parallel to the trailing edge, near its level and two chords behind it, is given in column two of Table I., and plotted in Fig. 1. The exact z level of the experimental line was so chosen as to give maximum upwash at the point H, corresponding nearly enough with maximum slope of the line JH. The nature of the readings clearly indicates the existence in some form of Lanchester's vortex with its centre at the point J and with a core—0.6in. diameter—of fluid having approximately uniform angular velocity.

TABLE I.

MODEL A.

(Span = 24in., Chord = 4in., Speed = 31.3ft./sec.)

Distance outside wing tip (inches).		Angle of downwash (degrees).
— 12.0	...	6.1
— 10.0	...	5.9
— 8.0	...	6.3
— 4.0	...	8.1
— 2.6	...	10.6
— 1.8	...	14.3
— 1.4	...	19.9
— 1.2	...	25.7
— 1.0	...	34.4
— 0.95	...	35.2
— 0.9	...	34.0
— 0.8	...	26.4
— 0.7	...	22.2
— 0.65	...	0.0
— 0.6	...	—5.0
— 0.5	...	—20.7
— 0.4	...	—34.0
— 0.35	...	—35.0
— 0.3	...	—33.0
— 0.2	...	—28.7
— 0.1	...	—24.5
0.0	...	—21.3
0.4	...	—13.9
1.0	...	—9.4
2.0	...	—6.3
3.0	...	—4.5
4.0	...	—3.4
5.0	...	—2.5
6.0	...	—1.8

It may be remarked that the form of such a curve—apart from the magnitudes of the readings—may be insufficient to decide on the existence of a columnar

vortex. Thus, immediately outside the wing tip of some rectangular acrofoils, a crosswash curve of the form in Fig. 1 may be associated with a continuous upwash curve, showing uniform shear without rotation in the sense of eddying. In the present case, a rough exploration of crosswash along a line at right-angles through *J* checked the deduction to be derived from a quantitative consideration of the readings.

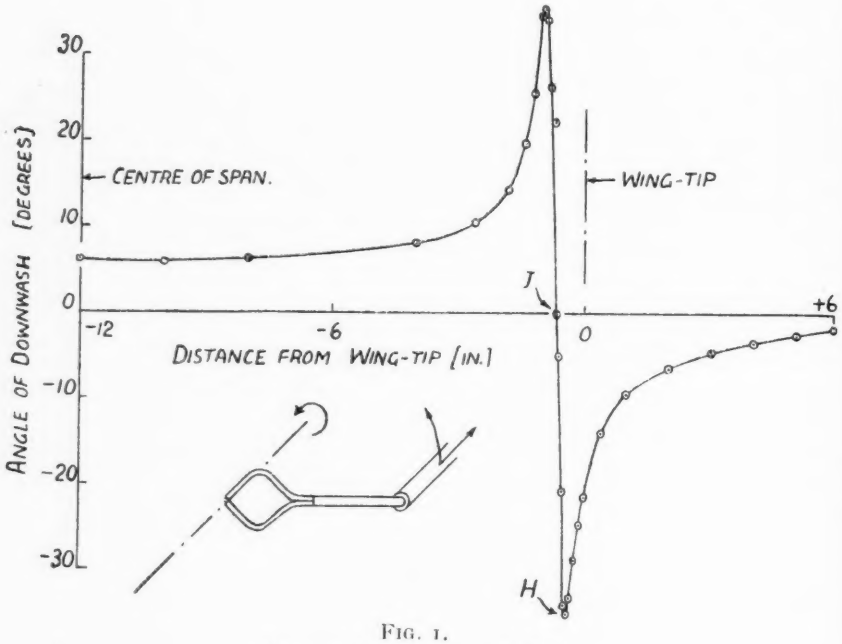

FIG. 1.
Downwash angles, Aerofoil A.

To examine the details of the vortex, it transpires that the simplest point of view is probably sufficient. More theoretical difficulties exist, however, than may appear at first sight, and a preliminary discussion is needed.

In the case of non-turbulence, Lamb teaches from Stokes that the spinning about its axis of a long circular cylinder in a viscous fluid initially at rest sets up a circulatory motion, the angular velocity of which, when steady, is inversely proportional to the square of the radius. Stresses now exist which impose a couple on the cylinder, but the form of the motion is the same as with an infinite rectilinear vortex in an inviscid fluid, except as to effects possibly arising from the question of the stability of a fluid core. A hint may thus be conveyed as to the manner in which viscosity, together with the aerofoil, generates the eddy, but whether, far past the critical speed, the average form of the eddy is still similar to that of an inviscid vortex remains an interesting question. Moreover, the superposition of the general stream, causing the particles to move on the average in helical paths, changes the direction and magnitude of the stresses. Added to this, the circulation depends in part on the history of the vortex pair and of other rotations, which may exist elsewhere without giving rise to distinct cored vortices.

Looking at the problem from the standpoint adopted by Levy,* complete information regarding the distribution of vorticity at any instant would enable the instantaneous values of the upwash readings to be calculated without reference to viscosity, since the experimental points are far removed from a boundary. The same might be more or less true of average values, and under very simple conditions the process would be reversible. Nothing like the experimental knowledge theoretically required is available, of course. On the other hand, a glance at the numerical values in Table I. shows that the eddies at the wing tips are of outstanding importance in determining the motion adjacent to them.

There are thus some theoretical grounds for applying inviscid methods to the present problem, though the process clearly remains to be justified or condemned by experiment. The first assumption involved is that modifications of the vortices in the x direction (parallel to the undisturbed stream) due to viscosity are negligible in their effect on the upwash readings. Other assumptions require to be made regarding the configuration of the vortex in three dimensions, and it is necessary to choose between these. For this purpose, the principle has been adopted that the circulation due to the vortex round its core shall be independent of the path. Only the outer part of the vortex is examined so as to minimise as far as possible the superposed velocities arising from imperfectly known causes. The x component u of the velocity q is assumed constant in this region and equal to its undisturbed value V.

The simplest assumption as to configuration is to suppose that the effects of undetermined factors balance out in the experimental region, so that the whole velocity of upwash (w) is due to the trailing vortex pair. Neglecting also the limitation of length upstream, the problem becomes two-dimensional, and w outside the core is given by

$$w = (K/2\pi)\,[1/y - 1/(l + y)] \quad . \quad . \quad . \quad . \quad (1)$$

where K is the strength of the vortex (equals twice the angular velocity of the core, or the circulation round it), l is the distance apart of the vortices, and y is measured outwards from the centre of the core.

To allow for the walls of the channel, it is theoretically necessary to introduce a complicated image system arranged in doubly-infinite columns and rows. A great simplification may be obtained, however, if we accept the notion that the substitution of a circular channel for the square one actually used would not affect the upwash observed by much more than the experimental errors. In this case two images are sufficient, each distant $c^2/\frac{1}{2}l$ from the centre of the channel, of which the radius is c. The formula (1) is then amplified to

$$w = (K/2\pi)\,[1/y - 1/(l + y) + 1/(2c^2/l + l/2 + y) + 1/(2c^2/l - l/2 - y)] \quad (2)$$

and we may choose the arbitrary value 2.1 (ft.) for c so as to give reasonable coincidence over a fair space to the supposed and actual walls.

Now suppose the vortices only extend upstream as far as the mid-point of the chord, and are there attached to the surface. To calculate the velocity we have to go back to the general formula

$$\delta q = (K/4\pi)\,(\sin\beta\delta s/r^2) \quad . \quad . \quad . \quad . \quad (3)$$

where r is the distance of a small length δs of a vortex from the point at which q is required, and β is the angle between r and δs. A suitable application of this gives

$$w = (K/4\pi)\,\{(1/y)(1 + x/\sqrt{\{x^2 + y^2\}}) - [1/(y + l)](1 + x/\sqrt{[x^2 + (y + l)^2]})\} \quad (4)$$

x being the distance of the yz plane of the measurements behind the mid-point of the chord. As before, an approximate allowance for the walls of the channel may readily be added.

* Levy, AERONAUTICAL JOURNAL, Vol. XXIII., page 335 (1919).

As a final alternative, following Prandtl, let us assume the aerofoil to be replaced by a transverse vortex joining the upstream ends of the trailing vortex pair. We get, from (3)

$$w=(K/4\pi)\left[(1/y)(1+x/\sqrt{\{x^2+y^2\}})-\{1/(y+l)\}(1+x/\sqrt{\{x^2+(y+l)^2\}})\right.$$
$$\left.-(1/x)\{(y+l)/\sqrt{x^2+(y+l)^2}-y/\sqrt{(x^2+y^2)}\}\right] \quad . \quad . \quad (5)$$

but correction for the channel is more cumbersome. The simple nature of the substitution for the aerofoil requires x to be sufficiently great, and this we shall have to assume to be satisfied by the experiments, for the time being. Another assumption of (5) is that K is not only constant along the trailing members of the filament (which may, if desired, be considered re-entrant by supposing a second transverse member at infinity), but has also the same value along the transverse member. This question will be referred to again.

The first three columns of Table II. give values of $K/2\pi$ as calculated by formulæ (1), (4), and (5) from some of the upwash readings of Table I. Neglecting the walls of the channel leads to an apparent increase of K with y in every case. All the methods give approximate agreement close to the core, and little difference results, over the present range of y, in taking into account the limited extent upstream of the vortices. The substitution for the aerofoil, however, doubles the variation and there appears little hope of obtaining a constant value for K by the use of such formula as (5). Returning, therefore, to the method of (1) and (4), but allowing for the walls of the channel, we obtain the last two columns of the table showing that the image corrections secure fair constancy for K.

TABLE II.

APPLICATION OF THE FORMULÆ. MODEL A.

y (chords) from centre of core.	$K/2\pi$ from formulæ. (1)	(4)	(5)	(2)	(4) modified.
0.112	0.66	0.66	0.67	0.65	0.65
1.41	0.81	0.80	0.96	0.63	0.69

Comparing the last two columns, the small advantage suggested in favour of (2) was verified in another example of greater y range taken from Table IV. below. This showed that the two-dimensional assumption gave practically a unique value for K. Table V. below clears up the question raised in choosing the points for Table II. Formula (2) is therefore used in the following work to reduce the readings. The failure of (5) is of course no comment on Prandtl's aerofoil theory, for the formula may be incomplete as to important details. For instance, a number of similar but narrower and weaker vortex filaments might lie between the two trailing vortices, so arranged as not to give rise to cores. These might build up the transverse vortex towards the centre of span and yet, on the whole, yield an upwash in the experimental region sufficient to balance—or partly so—the downwash from the wing.

In applying (2) to the figures obtained from Model A it has been chosen to fit best the points adjacent to the core. The calculated curve of upwash so obtained passes fairly through the distant points but somewhat misses intermediate values, as will be seen by comparing columns 3 and 4 of Table III. The value obtained for K in this way is 4.08 (ft./sec. units). On the whole the agreement is not bad and, in view of the greater success obtained later, the discrepancies may fairly be regarded as experimental errors, until further work is available for comparison.

The next step in the work was to examine the extent to which the assumption of constant u is valid. This was carried out by measurements of resultant

velocities in the following manner. A small T-shaped arrangement of two fine hypodermic tubes was used, one pointing upstream to measure the dynamic head and the other pointing downstream immediately behind it. The instrument was first calibrated for speed and inclination to the wind and then drawn through the centre of the vortex. The readings obtained, corrected for previously observed angle variation, are given in column 5 of Table III., where they may be compared with values obtained from the calculated values of w (column 6). The agreement, which is satisfactory to within a short distance of the core, may alternatively be inspected from the values of u as deduced from the measured angles and resultant velocities (column 7).

TABLE III.

VORTEX OF MODEL A.

1.	2.	3.	4.	5.	6.	7.	8.	9.
	Expertl.	Expertl.	Calcd.	Expertl.	Calcd.	Expertl.	Expertl	Calcd.
y	angle.	w	w	q	q	u	p	p
in.	deg.	ft./sec.					lb./sq. ft.	
0.0	0.0	0.0	0.0	36.0	31.3	36.1	1.57	1.60
0.05	5.0	2.75	4.33	—	—	—	1.51	1.58
0.15	20.7	11.8	13.0	—	—	—	1.45	1.40
0.25	34.0	21.1	21.7	36.8	38.1	30.5	0.94	1.045
0.30	35.0	21.9	26.0	37.5	40.7	30.7	0.73	0.80
0.35	33.0	20.3	22.2	36.7	38.4	30.8	0.55	0.59
0.45	28.7	17.1	17.3	35.5	35.8	31.1	0.36	0.355
0.55	24.5	14.3	14.2	34.4	34.4	31.3	0.265	0.24
0.65	21.3	12.2	12.0	33.6	33.5	31.3	0.25	0.17
1.05	13.9	7.75	7.4	32.0	32.2	31.0	0.10	0.065
1.65	9.4	5.2	4.7	31.5	31.65	31.1	0.025	0.026
2.65	6.3	3.45	2.95	31.5	31.45	31.3	0.015	0.010
3.65	4.5	2.45	2.15	31.4	31.38	31.3	0.005	0.005
4.65	3.4	1.85	1.7	31.3	31.35	31.3	—	—
5.65	2.6	1.4	1.4	—	—	—	—	—
6.65	1.9	1.05	1.2	—	—	—	—	—

Another check and point of interest is provided by an examination of the pressures measured separately in the backward-turned tube of the T-shaped instrument. After correction for angle and speed in accordance with a separate calibration, these give the static pressure distribution through the vortex. The corrected experimental pressures are tabulated in column 8 of the same table, while in the last column are given the corresponding values calculated from K in accordance with the well known formulæ for an inviscid fluid, viz.,

$$p = \rho K^2/8\pi^2 r^2 \text{ and } p = (\rho K^2/4\pi^2 a^2)(1 - r^2/2a^2) \quad . \quad . \quad (6)$$

for outside and inside the core, respectively. In (6) p is the pressure drop relative to the pressure of the undisturbed stream, at a radius r (equals y) and a is the radius of the core. In view of the special need in the application of (6) for accurate estimations of K and a, the agreement with the experimental results is better than might well be anticipated on experimental grounds.

The curves (*a*), (*b*), and (*c*) of Fig. 2 are plotted from columns 4, 6 and 9 of Table III., and are therefore theoretical, referring to w, q and p respectively. The observations are shown as points. The values most distant from the core are omitted.

The measured velocity at the centre of the core has a peculiar interest. It has closely the value which would be produced from rest (at the pressure of the

undisturbed stream) by the observed pressure reduction. If the central portion of the core could be traced upstream to an originating point such that the vortex motion had not there been formed, the mechanical energy at that point would thus be $\frac{1}{2}\rho u^2$ less than in the undisturbed stream.

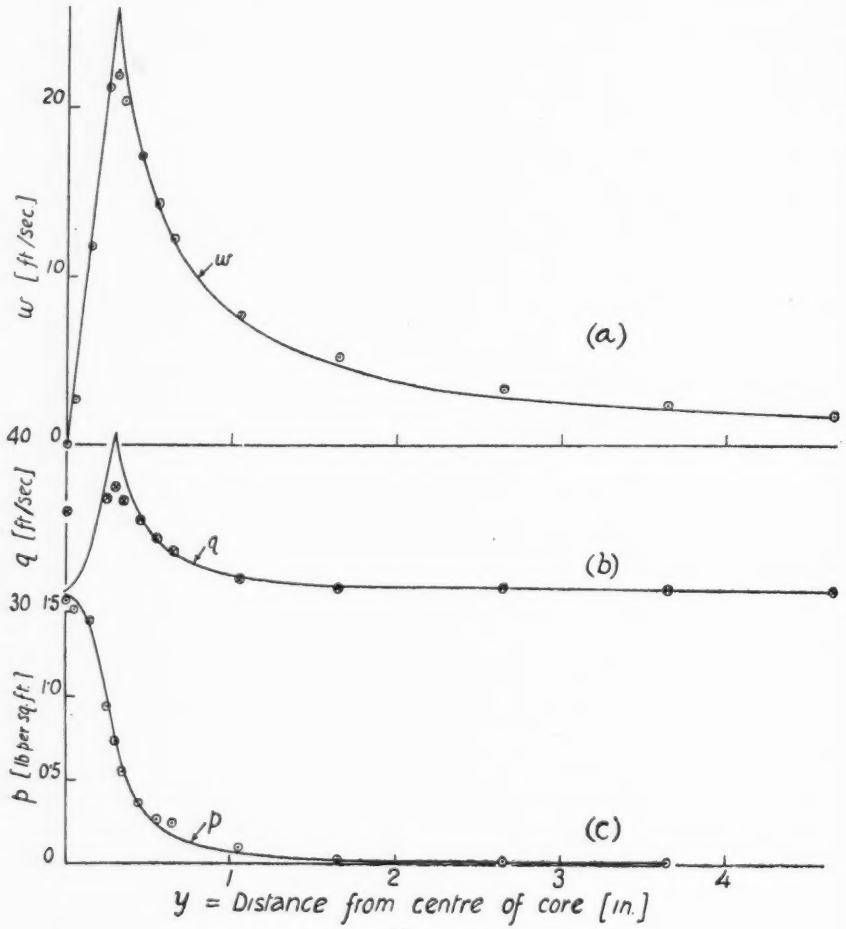

FIG. 2.

Details of vortex of Model A.

Variation of Vortex Strength

The results so far obtained encourage the belief that the present method of analysis is sufficient and that a viscous vortex exhibits to perhaps a remarkable degree the properties developed by Helmholtz. Further work is justifiable, and proceeding on the same lines, the increase of the strength of the vortices with incidence has been determined for a square-ended aerofoil B of R.A.F.15 section, measuring 18in. span by 3in. chord. The wind speed was 40ft./sec.

The observed downwash angles are given in Table IV., y being measured from the wing tip, for six angles of incidence. Values of w are compared in Table V. with those calculated from the best value of K in every case except the greatest angle. Agreement will be observed to be good. With the greatest angle (20deg.), the points were found to lie closely on a theoretical curve obtained by assuming a displacement of the centre of the vortex from the measured position. The calculated values given were derived from this curve. The close agreement suggests that the advancing measuring apparatus, on approaching the core closely, sufficed to push it inwards at this angle of unstable flow. Readings for 0 deg. were taken but are not included on account of known experimental inaccuracies. A core was formed, but there seemed reason to believe that either the external motion was not a vortex at this distance behind the aerofoil or a change of method was required for analysis. The readings were such as to suggest the possible applicability of equation (5).

TABLE IV.

MODEL B.

(Span = 18in., Chord = 3in.)

Angles of downwash in degrees. Speed: 40ft. per sec.

Distance outside wing tip (in.)	Angle of incidence (degrees).					
	3.1	7.1	10.2	13.2	17.0	20.0
— 9.0	2.8	3.8	4.7	4.4	2.8	—
— 7.5	2.4	3.65	4.4	4.4	2.3	—
— 6.0	2.4	3.75	4.7	4.8	4.8	—
— 4.5	2.5	4.0	5.2	5.9	7.6	5.0
— 3.0	2.85	4.6	5.8	6.8	9.8	12.3
— 1.5	3.5	6.15	8.6	10.4	13.6	18.6
— 1.2	4.4	7.9	11.1	13.3	17.2	23.7
— 0.9	6.9	12.1	17.3	21.0	24.5	31.8
— 0.75	10.0	16.4	22.2	28.5	31	33
— 0.65	—	19.6	26.5	31	32.5	27
— 0.6	13	21.1	27.5	31	32	25
— 0.55	10.6	21	26	30.5	29	15.5
— 0.5	8.6	17	19	24.5	27	4
— 0.45	— 4	7.5	3	17	24.7	— 11
— 0.35	— 11.5	— 10.5	— 16.5	— 9	— 6	— 22.8
— 0.3	— 12.4	— 18	— 22	— 22.5	— 20	— 25.3
— 0.25	—	— 20.8	— 25	— 26.5	— 24.2	— 25.6
— 0.2	—	— 20.4	— 24.3	— 29 .	— 27.8	— 25.5
— 0.15	— 10.2	— 19.0	— 23.3	— 29.5	— 29.7	— 25.0
— 0.1	—	— 17.2	— 21.1	—	— 29.4	— 23.9
0.0	— 8.4	— 13.6	— 17.3	— 23.5	— 25.4	— 21.1
0.15	— 6.6	— 12.1	— 14.5	— 17.9	— 20.0	— 18.2
0.3	— 5.5	— 10.0	— 13.2	— 15.4	— 16.6	— 15.4
0.6	— 3.9	— 7.3	— 9.6	— 11.3	— 12.5	— 11.9
0.9	— 3.1	— 5.8	— 7.3	— 8.7	— 9.4	— 9.2
1.2	— 2.5	— 4.4	— 5.8	— 7.0	— 7.6	— 7.3
1.5	— 2.1	— 3.8	— 4.8	— 5.7	— 6.1	— 5.9
3.0	— 1.2	— 2.2	— 2.7	— 3.2	— 3.4	— 3.2
4.5	— 0.8	— 1.6	— 1.8	— 2.2	— 2.3	— 2.2
6.0	— 0.6	— 1.2	— 1.4	— 1.6	— 1.6	— 1.6
7.5	— 0.5	— 1.0	— 1.1	— 1.3	— 1.4	— 1.3

The six values of K are given in Table VI. together with those of the lift and drag coefficients. Dimensional theory states that, for geometrically similar models, K will vary as the product VL, subject to a scale correction. The quantity K/VL has therefore been tentatively denoted as the " Vortex coefficient " and is also tabulated. In Fig. 3, K is plotted against α and K/VL

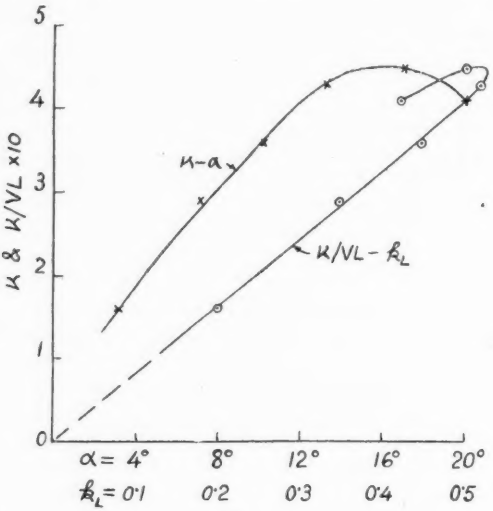

FIG. 3.

Increase of strength of vortex of Model B with incidence.

against k_L. It will be seen that K is linearly related—or nearly so—to the lift over a wide range of flying angles, and reaches a maximum value at a somewhat greater angle than that of maximum lift.

The observations of downwash behind the centre of span at the larger angles (Table IV.) serve to indicate the well-known early failure of the middle portion of the aerofoil. They are not, however, a direct guide to loss of lift, since the downwash curve in the z direction is found in these circumstances to have a double peak instead of the single one characteristic of smaller angles. Thus at 20 deg. and 1.5 chords from the symmetrical plane the minimum downwash was observed to be —0.4 deg. at a little distance below the level of the trailing edge of the aerofoil, while the two turning values were about +7.0 deg. and 2.6 deg., at 0.5 chords above and below this level respectively.

The whole of the foregoing work was carried out at 2.0 chords behind the wing. The results obtained both numerically and in form may be expected to depend on this distance for reasons which have already been set out and also for the time required by the wing to produce the vortex. A remarkable feature, however, is that this variation has been found to be small over a considerable range. Some measurements on this question are given in Table VII. Even so close as 0.5 chord behind the trailing edge it was unexpectedly found that formula (2) was still the best for the readings and agreed with them well, giving the vortex to be almost in full strength. The small mutual approach—or bending inwards of the cores—may also be gathered from the table. A rather rough test has been made at 12 chords behind an aerofoil without revealing any great modification. It seems then that with this class of aerofoil the specification of down-

stream distance is unnecessary, within limits. For a few chords behind the aerofoil, but away from its immediate neighbourhood, the cores are just within the wing tips, their centres being separated by 0.95 of the span. This ratio is approximately independent of the angle of incidence, and is about the same for the two models. Prandtl's work gives 0.79 for this number at infinity.

TABLE V.

VELOCITIES DUE TO VORTICES OF MODEL B.

(w deduced from angle of stream, w^1 calculated from K.)

y^1	Angle of incidence (degrees). 3.1		7.1		10.2		13.2		17.0		20.0	
(in.)	w	w^1	w	w^1	w	w^1	w	w^1	w	w^1	w	w^1
0.3	3.85	3.96	7.06	7.54	9.40	9.70	11.02	11.19	11.9	12.32	11.0	12.9
0.6	2.73	2.82	5.13	5.28	6.77	6.72	8.0	7.84	8.87	8.50	8.44	8.53
0.9	2.17	2.18	4.07	4.07	5.12	5.12	6.12	6.01	6.63	6.47	6.48	6.36
1.2	1.75	1.77	3.08	3.28	4.08	4.14	4.92	4.86	5.34	6.21	5.12	5.05
1.5	1.47	1.49	2.66	2.75	3.36	3.48	4.00	4.07	4.28	4.37	4.12	4.18
3.0	0.84	0.82	1.54	1.51	1.88	1.88	2.24	2.23	2.38	2.36	2.24	2.22
4.5	0.56	0.56	1.12	1.03	1.26	1.29	1.54	1.53	1.61	1.61	1.52	1.52
6.0	0.42	0.43	0.84	0.79	0.98	0.98	1.12	1.16	1.12	1.22	1.12	1.14
7.5	0.35	0.35	0.70	0.64	0.77	0.79	0.91	0.95	0.98	0.99	0.92	0.92

(y^1 = distance outside wing tip, velocities in ft. per sec.)

TABLE VI.

COEFFICIENTS OF VORTICITY, LIFT AND DRAG. MODEL B.

Angle of incidence (degrees)	K	K/VL	k_L	k_D
3.1	1.6	0.16	0.200	0.0125
7.1	2.9	0.29	0.348	0.024
10.2	3.6	0.36	0.445	0.0375
13.2	4.3	0.43	0.515	0.054
17.0	4.5	0.45	0.500	0
20.0	4.1	0.41	0.420	

(L = chord in ft.)

TABLE VII.

MODEL B.

Angle of incidence = 13.7°.

Distance behind trailing edge (chords)	K (ft./sec. units)	l
0.5	3.85	1.46
2.0	4.25	1.43
4.0	4.35	1.41

Applying dimensional theory we observe that both l and a should vary with the scale, subject to a viscosity correction. The observed core diameter for Model A suggests the possibility of a large core being present with the full-scale machine. The core is much smaller, however, than has been advanced on theoretical grounds. Estimation of the size of the core which may be derived from Table IV. for Model B should be treated as only of an approximate nature.

In contrast with angles well outside the core, those within it in the case of this aerofoil appeared to be very unsteady and repeat readings were wide apart. Plotting Table IV. showed the estimated averages to be a little incoherent, so a static pressure check was made. From this it was calculated that the diameter of the core at 5 deg. should be nearly 0.5in., instead of the considerably smaller value indicated by the downwash readings. Owing to the unsteadiness the analysis of the readings has not been carried quite so close to the core as with aerofoil A—where the readings were steady with the same damping. The estimations for K may be slightly on the high side on this account, but it is surmised that the apparatus may have been too clumsy for the smaller model and have caused the cores to behave in the manner of a spring on passing the measuring head through them, leading to an underestimate of size.

On the whole, the core diameter appears to increase somewhat with increase of incidence, though no clear interpretation is possible. Some interesting conceptions may be roughly formed from the vortex readings at say 17 deg. At a radius of 0.1in. from the centre of the core, the mean rotational component of speed is over 17ft./sec. This makes w about 2,000, so that the centre of the core is rotating at over 300 revs. per sec. The drop in pressure at the centre should exceed ρu^2 and the velocity there should be some 50 per cent. in excess of the general speed, applying the results obtained from Model A.

Impulse and Energy Loss

The equations of impulsive motion of a real fluid being identical with those of inviscid fluid, we may apply the theory of impulse of the latter case to seek a partial balance between the force on the aerofoil and the rate of change of momentum imparted to the stream. The rate of change of impulse required to generate continuously the vortex pair is (neglecting the cores) $K\rho Vl$. Equating this to a part of the force, say Z,

$$Z = K\rho Vl \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Alternatively (7) might have been inferred from dimensional theory in the form

$$Z = K\rho Vlf\,(Vl/\nu,\ K/\nu) \quad . \quad . \quad . \quad . \quad (8)$$

The same formulæ are obtained for the lift due to cyclic flow about a solid transverse core, if of length l, in the stream.

Using (7), Z may readily be calculated from the experimental results, yielding row 2 of Table VIII. Row 1 gives the angles of incidence, row 3 the lift in lb. of the aerofoils as determined on the wind channel balances, and row 4 the ratio Z/lift. The last column refers to the Model A, the others to the second aerofoil B.

TABLE VIII.

	Aerofoil A.						Aerofoil B.
α (degrees) ...	3.1	7.8	10.2	13.2	17.0	20.0	8.0
$K\rho Vl$ $(=Z)$ ...	0.216	0.394	0.496	0.584	0.613	0.569	0.58
Lift	0.285	0.495	0.633	0.732	0.711	0.598	0.97
Z/lift	0.76	0.80	0.78	0.80	0.86	0.95	0.60

According to the Prandtl aerofoil theory, Z is inclined backwards a little from the direction of the lift, and the present experiments do not throw any light on this question. Considered numerically, however, the results in the last row of the table would evidently be little changed by a slight inclination of Z. In the case of the aerofoil B the impulse per sec. required for the vortex pair amounts to about 80 per cent. of the lift at the smaller angles, while at the greatest angle almost all the lift may be traced in the stream in this manner. The proportion is not constant for different aerofoils.

If Z is at an angle to the lift, what has been called the "induced drag" arises from the generation of the vortices. Entirely apart from this question, there are two causes by which the vortices contribute to the drag of the aerofoil. The first is directly traceable to the presence of cores, while the second follows from the difference of the external motion, specially near the cores, from the inviscid form.

Separating the effects of the vortex from other components of the flow, the difference due to the vortex in the rate at which mechanical energy crosses the yz plane is given by (in accordance with the principles discussed elsewhere)*

$$E = \int u.D.2\pi r dr \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where u is the *disturbed* velocity component, and D the pitot head difference caused by the vortex. The assumption: u=its undisturbed value outside the core leads at once, on substituting from (6) to $E=0$ for that region. In fact, the first equation of (6) is obtained by assuming the equivalent of $E=0$ outside the core.

Inside the core, however, $q = \sqrt{(u^2 + w^2r^2)}$ or for the velocity to be continuous at $r = a$,

$$q = \sqrt{(u^2 + K^2r^2/4\pi^2a^4)} \quad . \quad . \quad . \quad . \quad (10)$$

Hence (9) gives $E = \rho u K^2/8\pi$, an expression independent of the radius of the core. Equating this to the rate at which work is done by the fluid against the corresponding portion R of the resistance of the aerofoil, we get for the two vortices (neglecting mutual effects)

$$2R = \rho K^2/4\pi \quad . \quad . \quad . \quad . \quad . \quad (11)$$

For the Model B and for angles from 7 deg. to 13 deg., (7) gives resistances amounting to about 5 per cent. of the whole aerofoil.

TABLE IX.

ENERGY LOSS. MODEL A.

y (in.)	$lq\,(D)$	$lq\,(D)$ (u const.)
0.0	42.8	50.0
0.25	15.1	15.3
0.3	6.9	0.0
0.35	3.5	0.0
0.45	0.9	0.0
0.55	0.7	0.0
0.65	2.3	0.0
1.05	1.45	0.0
1.65	0.35	0.0
2.65	0.03	0.0
3.65	— 0.06	0.0

Referring back to Model A, some account may be taken of the actual variation of u with radius. Table IX. gives experimental values of $u.D$ along the experimental line. Comparison with the corresponding values for u constant (column 3) shows the energy differences involved. On multiplying the element of column 2 by $2\pi r$ and integrating, the result comes out as 85 per cent. of $\rho K^2/8\pi$, the speeding up of the stream at the centre somewhat decreasing the loss. The energy loss is not confined to the core in the actual case. If the

* I regret that the paper dealing with this question is not yet published.

external loss is assumed symmetrical and as given along the experimental line, the whole works out to 0.0037 for a single vortex, including the core, or more than twice the value given by (11). It appears reasonable to estimate that about 10 per cent. of the whole drag of an aerofoil at usual angles of incidence is directly due to the flow in and around the cores of the vortex pair.

It is hoped that the work described has shown that the wing tip vortices are amenable to accurate investigation. Only the fringe of the subject has been touched and further experiments, particularly on the largest scale feasible, should be productive of results of importance in relation to aerofoil theory. The relation of the present results to Prandtl's theory is many-sided and has scarcely been approached from any direction, efforts being rather directed to sketching the observed motion as clearly as possible. A single aspect forms the subject of a separate note.

There is an evident practical advantage of some importance in ridding a wing of cored vortices, where this may be effected without loss of lift. While, once formed, their strength is intimately connected with lift, the details of the relationship are obscure, and, as may be gathered from some remarks at the beginning of this paper, recent attempts to obviate them have met with success. Difficulties may be encountered, however, in carrying out this policy. It would be surprising if cored vortices were not common with full-scale aircraft. Their general presence recognised, the knowledge should be of utility. On the one hand they may have an occasional effect on design and construction,* and in applied channel testing, on the other an effort might well be made towards recovering a proportion of the power wasted.

The author has pleasure in acknowledging the financial provision of the Department of Scientific and Industrial Research, which allowed more time to be spent in completing the work and preparing the account of it than would otherwise have been possible. He has also had the great advantage of being able to consult with Prof. L. Bairstow.

* The writer has come across an example of this in practice.

NOTE ON THE EXPERIMENTAL ASPECT OF ONE OF THE ASSUMPTIONS OF PRANDTL'S AEROFOIL THEORY

BY N. A. V. PIERCY, D.SC.

Mr. Glauert's paper "Theoretical Relationships for the Lift and Drag of an Aerofoil Structure" strikes a bold note in asking us to discuss the fundamental principles of Prandtl's aerofoil theory rather than its detailed application. His suggestion is a particularly good one, however difficult the task. Justification of the general truth of the suggestions brought forward by Lanchester, and so ably developed by Prandtl and his colleagues and others, have frequently been afforded by the application of the theory, and similar support only awaits the seeking. But, to achieve its greatest utility, the theory should enable us to extrapolate experimental knowledge with confidence; an example is provided by the question as to what limit, if any, exists to the camber of wings on economic grounds and apart from burble considerations. An approximate tally—rough, in some cases—of the theory with observation over a limited range is inadequate to establish it sufficiently for such a purpose. The theory may indicate with truth the existence of, say, a limit to a certain development, but its numerical calculation of the limit may yet be widely wrong.

The examination of the fundamental assumptions of the theory is a first step towards the inclusion of terms, if this be necessary and feasible, which would make the theory account accurately for well-established observation, and towards framing such terms on a rational basis so that generality may not be lost. It may be that this process would yield results more quickly than the preferable though more difficult onslaught of the viscous equations.

The present note is confined to the assumption whereby vorticity in a strictly limited region is in effect substituted for part of the general action of viscosity. The assumption is especially interesting, for in certain circumstances the form of a vortex in a real fluid is very similar to the familiar theoretical form in one devoid of viscosity. In a recent number of the Journal, Dr. Levy worked out for us a fascinating example of this. The vortex pair formed quickly by some aerofoils provides another example. The success of such work depends on knowledge of the situation or distribution of the vorticity. Prandtl's assumption in this connection is that the vortices are confined to a thin sheath containing the aerofoil and uniting at the trailing edge into a trailing vortex sheet which remains thin behind most of the span, but rolls up into the trailing vortex pair. That part of the action of viscosity which the vortex system replaces is also therefore confined to a thin sheet. I have stated the assumption in this way because, in expressions given for the drag of an aerofoil, the theory provides a term for a certain "profile" drag which is not susceptible to calculation by vortex methods. In Mr. Glauert's paper, this term is attributed entirely to viscous forces. It is not to be associated with skin friction, for it is so large as to include a considerable part of the normal pressure integration. The notion that the theory deals with only a part of the system of flow is important and needs emphasis.

The presence of an additional viscous system, which in the usual type of aerofoil is of major importance so far as the drag is concerned, places a difficulty in the way of examining the fundamentals of the theory in an experimental light. With this reservation, however, it may readily be shown firstly that a limited region indeed exists where viscosity produces striking effects, but that this sheath is not of the evanescent thickness contemplated, and in fact is better described as a "bag." Secondly, it appears that viscosity also leaves, though with a

lighter hand, a widespread stamp on the more distant motion. Experimental evidence is brought forward in support of these statements and may be examined below.

In view of the far-reaching effects of viscosity that we actually find, the feasibility of dividing the flow into independent viscous and vortical components, and not concerning ourselves with details of the former, may be questioned. Consider for instance what takes place near the critical angle. It has recently been shown that the strength of the trailing vortex pair of some aerofoils attains a maximum value at a substantially greater angle than that of maximum lift. The burbling at the central part of the aerofoil associated with loss of lift is apparently to be traced to viscous action since it finds no place in the vortex theory; in fact, the vortices appear to be prepared to continue growth with incidence at the angle at which failure first occurs. We may seek an explanation in several ways. We may infer that at this angle the two wing-tip vortices are not joined together by transverse members along the aerofoil—in which case it is not necessary for them to be so joined at a smaller angle—or that they may be joined in such a manner as not to give cyclic lift. As a third alternative, we may suppose that cyclic lift may be destroyed to a considerable extent by viscous effects, when it seems reasonable to conclude that cyclic lift is not immune from viscosity at smaller angles.

To such an argument the objection may be raised—it has been raised in the past—that the cyclic flow theory has nothing to do with an aerofoil at an angle of burble. But that is beside the point. The question is whether we can afford to neglect at 8deg. incidence, say, a factor so powerful as to be able to overthrow the vortex system at, say, 16deg. Should we not rather conclude that at any angle viscosity is playing an essential and important rôle in the whole system of flow?

With these introductory remarks I invite the attention of the Society to some experimental information relating to the nature of the flow over the upper surface of an aerofoil near its centre of span. Some measurements in three dimensions of the size and shape of the special viscous "bag" are added. Details of the motion within the bag have been studied, and it is hoped to describe these shortly in a separate paper.

On the Motion Above the Aerofoil in the Symmetrical Plane

The aerofoil A had 14 per cent. and 6 per cent. camber for the upper and under surfaces respectively. It measured 1.5in. chord and 13.4in. from tip to tip, and had long graded wing tips. Set at $\alpha = 9.5°$ in a stream of $V = 41$ft./sec. in a 4ft. channel, it was not quite at, though very near to, its critical angle. The object in employing so large an angle was to obtain measurements inside the specially viscous layer. The observations recorded in Table I. (*a*, *b*) were obtained along two lines drawn upwards, parallel to the lift, at distances of about one-third and two-third chords from the leading edge. Fine pitot and static tubes were arranged at short equal distances on either side of the centre of span. These were oriented so as to lie approximately along the streamlines, and the relative distance from the model was varied by traversing the latter. In the table the first column gives the distance of the centres of the tubes from the local surface (z); the second, the loss of pitot head (D); the third, the reduction of static pressure; the fourth, the variation of velocity. Some of the observations are plotted in Fig. 1. The points surrounded by broken circles for $z = 0$ were found by assuming that on the surface the velocity was zero and the static pressure the same as at a short distance away. The extension of the curves in this way to $z = 0$ seems reasonably consistent with the readings.

TABLE I.

FLOW OVER UPPER SURFACE OF AEROFOIL A.
(Chord = 1.5 in., Speed = 41 ft./sec.)

(*a*) 0.33 *chord behind leading edge.*

Distance above local surface.		Pitot loss.	Pressure drop.	Velocity factor.	Energy flow.
in.	chords.	$\div \rho V^2$	$\div \rho V^2$		$\div \rho V^3$
0.07	0.047	0.008	0.468	1.385	—
0.15	0.10	0.008	0.442	1.365	—
0.20	0.133	0.008	0.433	1.36	—
0.32	0.213	0.008	0.370	1.315	—
0.37	0.247	0.008	0.327	1.28	—
0.53	0.353	0.006	0.261	1.23	—
0.85	0.567	0.008	0.180	1.16	—
1.05	0.70	0.008	0.148	1.13	—
1.20	0.80	0.006	0.134	1.12	—
1.70	1.133	0.006	0.093	1.085	—
2.2	1.467	0.006	0.072	1.065	—
2.7	1.80	0.006	0.055	1.048	—
3.7	2.47	0.003	0.033	1.030	—
7.0	4.67	0.006	0.014	1.008	—
		(*b*) 0.67 *chord behind leading edge.*			
0.00	0.00	—	—	—	0.50
0.07	0.047	0.68	0.310	0.51	0.592
0.19	0.127	0.128	0.308	1.165	0.066
0.26	0.173	0.088	0.312	1.205	— 0.013
0.43	0.287	0.008	0.275	1.24	— 0.110
0.60	0.40	0.004	0.244	1.215	— 0.104
0.78	0.52	0.005	0.207	1.185	— 0.087
0.95	0.633	0.009	0.176	1.155	— 0.067
1.45	0.967	0.006	0.117	1.105	— 0.046
1.95	1.30	0.005	0.083	1.075	— 0.031
2.45	1.63	0.005	0.066	1.06	— 0.025
3.45	2.30	0.005	0.036	1.03	— 0.010
4.45	2.97	0.004	0.024	1.02	— 0.006
7.0	4.67	0.003	0.011	1.008	— 0.001

Several noteworthy features may be immediately noticed. The pitot head shows a small but measurable loss extending probably to seven chords or so from the aerofoil. As the model is approached, this increases only slightly until a point is reached where the head begins to drop at a great rate. At one-third C (chord) from the leading edge, this point is less than 0.05 C from the surface, but farther back it is about 0.2 C. The point lies on a fluid surface enveloping the aerofoil and (as will be shown) trailing behind it, which may easily be mapped out owing to the sharpness with which the drop begins.* It is proposed to call this fluid surface the "Pitot boundary," with the caution that it is not a boundary in the hydrodynamical sense, since fluid crosses it. Outside it, Bernoulli's theorem closely, though not exactly, applies; inside it, the practically constant pressure in the z direction shows the loss of energy in approximately that direction to be almost purely kinetic over the range of the observations. It would be sufficiently close for practical purposes to calculate the maximum velocity over a

* Mr. Harris Booth first suggested to me the existence and importance of the Pitot boundary. He also added the note that is given above as a caution and which differentiates it from, say, the slip-stream boundary used in airscrew theory.

position on the contour of a wing by applying Bernoulli to the normal pressure. Alternatively, a fair idea of the normal pressure at a position on the after-part of the upper surface of a wing could be obtained without drilling and coring a model by the more ready expedient of laying a fine static tube on the surface.

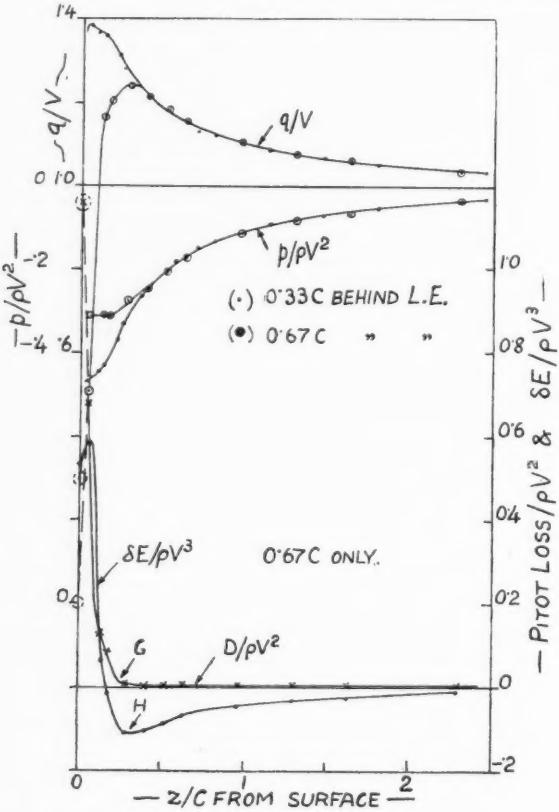

FIG. 1.

Flow above centre of span, Model A.

The pitot head gives the whole mechanical energy of the stream provided we neglect the possibility of an increment due to finely-divided rotations. The loss of head at distant points, where the rotation must be small, is a sufficient indication that in the velocity rearrangements caused by the wing, some of the mechanical energy is converted into heat before the experimental line is crossed. This loss must be traced to viscosity, and is seen to be widespread.

The change in the flow of energy across the line Cz is, of course, not given by the pitot head; but, if we call it E, by the expression :—

$$E = \int \{ lq \cdot D + \tfrac{1}{2}\rho V^2 (lq - V) \} \, dz \qquad . \quad . \quad . \quad (1)$$

where lq is the x component of the changed velocity, initially V. Values of δE are given in the last column of Table I. (*b*), from which it will be noticed that the energy-flow loss increases from 0.5 ρV^3 on the surface (assuming the velocity

there to be zero) to 0.6 ρV^3 at a little distance from it, whence it decreases to a considerable negative value which only again approximates to zero about $5C$ away. The initial increase of δE *may* be associated with the production of spin, making the expression (1) incomplete. The value of E within the experimental limits is negative, the increase of fluid crossing the line per unit time being out of all proportion to the pitot drop. This of course is explained by the slowing up of the stream beneath the model as demanded by cyclic theory. The figures just considered are also plotted in Fig. 1 and illustrate that the cusp value of the local energy-flow loss, occurring at the point H, is displaced from the trace of the pitot boundary G; δE becomes negative well within the latter. It is important to recognise* that (1) may only be applied to investigate local changes in energy-flow produced by a wing, as will be seen when details of the motion within the bag come to be considered.

The velocity curves (and hence approximately the static pressure curves) practically overlie at a distance from the aerofoil. The extent to which this is true may be gauged from Table II. (*a*, *b*), where the observed increments of velocity, $q - V$, are in each case compared with values calculated from the single formula: $q - V = 6.1/(z + 0.233) - 1$, where z is in chords. The thickening of the viscous layer appears to cut short the building up of the velocity above the aerofoil, and only that pressure drop which has been generated up to this point of intervention is transmitted to the aerofoil surface to assist in lift. If the viscous layer were of uniform thickness, the pressure at both points of the surface would be about the same.†

TABLE II.

VELOCITY OVER AEROFOIL A.

(Details as in Table I.)

(*a*) 0.33 *C behind L.E.*			(*b*) 0.67 *C behind L.E.*		
z. (in.)	$q - V$ (ft./sec.) Observed.	From Formula.	z. (in.)	$q - V$ (ft./sec.) Observed.	From Formula.
0.07	15.8	20.8			
0.15	15.0	17.4			
0.20	14.8	15.7			
0.32	12.9	12.7			
0.37	11.5	11.7	0.43	9.9	10.7
0.53	9.4	9.4	0.60	8.8	8.6
0.85	6.6	6.6	0.78	7.6	7.1
1.05	5.3	5.5	0.95	6.3	6.0
1.20	4.9	4.9	1.45	4.3	4.1
1.70	3.4	3.5	1.95	3.1	3.0
2.2	2.7	2.6	2.45	2.5	2.3
2.7	2.0	2.0	3.45	1.2	1.4
3.7	1.2	1.25	4.45	0.8	0.9
7.0	0.3	0.25	7.0	0.3	0.25

It may be noticed from the nature of the formula given above that if we assumed a redistribution of speed in the channel following the introduction of the model, such that, apart from the cyclic flow variation, the velocity was increased everywhere along the experimental line by approximately 2 per cent. of the undisturbed speed, the speed points would lie on a hyperbola. It may be

* I regret that the paper dealing with this question is not yet published.

† It is therefore a little difficult to see that the pressure diagram along the chord is determined by the methods developed by Joukowski, and is not rather controlled by viscosity.

as well, therefore, to note that certain experimental errors were carefully avoided. Four gauges were used—one connected the exploring pitot with a distant pitot, another the exploring static with a distant static, a third the exploring pitot and static, while the fourth was used for the general speed. A certain degree of checking was therefore possible and was satisfactory, and further, the data for the free stream at the one point where all the measurements were taken were obtained by removing the model from the channel.

In general, the speed formula :—

$$(q - V)/V = (S/\sqrt{[z^2 + S^2]})\{A/(z + B) - C\} \quad . \quad . \quad (3)$$

where S is a fraction of the half-span, and A, B, C are constants for a particular aerofoil at one angle and scale, has been found to fit fairly several cases that have been tested. It is proposed to consider these next, and they will illustrate the use of the first factor of the R.H.S. and show that some such factor is necessary. It may immediately be seen, however, that the form chosen for the factor is consistent with vortex-wing theory. The factor also makes the velocity increment vanish at infinity.

Model B was a rectangular aerofoil, 2in. chord, aspect ratio=6, of the section shown in Fig. 2. Model C had the same section and chord as B, but

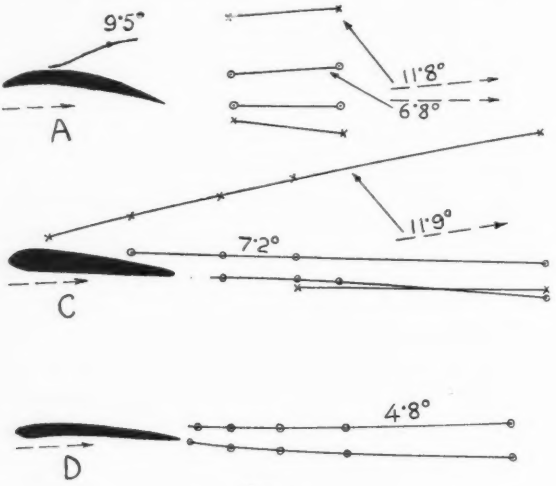

FIG. 2.

Pitot boundaries of three aerofoils behind centre of span.

the plan form shown in Fig. 3. Model D was a 3in. × 18in. rectangular aerofoil of R.A.F.15 section. All were tested in a 4ft. channel at an angle of incidence of about 4.8deg. and a general speed of 41ft./sec. Measurements of velocity were made along the line Oz in the symmetrical plane drawn vertically upwards from the uppermost point of the contour of the model as set in the channel. By this means the best effort possible was made to obtain the velocity distribution along a line orthogonal to the streamlines. The measurements are collected in Table III.

Taking S as half the span in the case of the rectangular aerofoils, the factor inside the brackets of (2) (xV) comes out for Model B as :—$3.9/(z + 0.24) - 1.0$, z being in chords. The " fit " of the formula is indicated with slide-rule accuracy below, the observations being labelled (M) and the calculations (F).

TABLE III.

Velocity Distribution above Three Aerofoils at 4.8°.

($V = 41$ ft./sec.)

	q/V		
z/C	Model B.	Model C.	Model D.
0.01	1.01	—	1.015
0.015	1.06	—	—
0.02	1.32	1.32	—
0.025	1.33	—	—
0.03	1.325	—	—
0.04	1.32	—	—
0.05	1.31	—	—
0.06	1.295	1.30	1.295
0.11	1.25	1.26	1.25
0.16	1.215	1.215	1.215
0.21	1.19	1.18	1.185
0.26	1.16	1.15	1.16
0.38	1.13	1.10	1.125
0.51	1.095	1.082	1.09
0.76	1.062	—	—
1.51	1.027	1.015	1.030
2.51	1.012	1.004	1.014

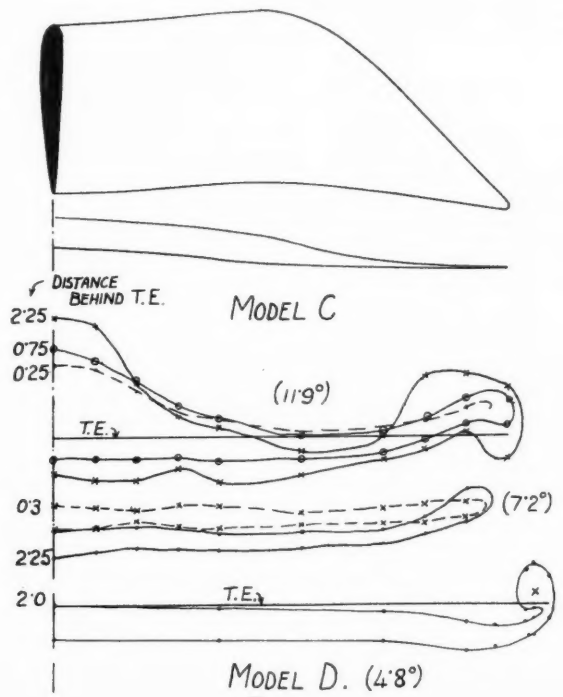

FIG. 3.

Sections through pitot boundaries of two aerofoils.

MODEL B. VELOCITY INCREMENTS IN FT./SEC.

z/C ...	0.02	0.025	0.03	0.04	0.05	0.06	0.11	0.16	0.21	0.26	0.38	0.51	0.76	1.51	2.51
$q-V$ (*M*)	13.0	13.5	13.4	13.1	12.8	12.1	10.3	8.9	7.7	6.6	5.2	4.0	2.6	1.1	0.5
$q-V$ (*F*)	14.0	13.7	13.4	12.9	12.4	12.0	10.1	8.7	7.7	6.8	5.3	4.1	2.8	1.1	0.3

It will be noticed that the formula is successful quite close to the aerofoil. If we took a shorter length for S, as would appear to be suggested by Prandtl's theory, the calculation for $z=2.51$ would be thrown somewhat further out, but the difference would not be great. The constants chosen for the formula are open to a certain variation depending on which part of the curve it is desired to fit most closely, but the value for the constant B requires to lie between 0.2 and 0.24, and the larger value appears to have advantages.

Proceeding to consider Model C, an essential difference is observable following the inclusion of wing tips in the span; the additive velocity—scarcely changed near the aerofoil—decreases more rapidly with distance. This illustrates the necessity for the first factor of (3). Taking S as the distance to the shoulder of the wing tip, or to the end of the central portion of constant section ($1.5\ C$), a change of the factor in the brackets (xV) to $2.9/(z+0.15)-0.8$ reflects the alteration. The substantially lower value for the constant B suggests that well-designed wing tips may enable a given pressure drop on the surface to be built up with less effect on the flow at a large distance, or that with the same effect at a distance a greater pressure drop will result at the aerofoil. The measurements are again compared with the values calculable and agreement is seen to be fair until the two points nearest to the aerofoil are reached.

MODEL C. VELOCITY INCREMENTS IN FT./SEC.

z/C	0.02	0.06	0.11	0.16	0.21	0.26	0.38	0.51	1.51	2.51
$q-V$ (*M*) ...	13.2	12.2	10.6	8.9	7.4	6.1	4.1	3.4	0.6	0.15
$q-V$ (*F*) ...	16.2	13.0	10.3	8.6	7.3	6.3	4.6	3.4	0.7	0.15

Finally, $3.3/(z+0.2)-0.6$ seems best to suit the results for Model D.

MODEL D. VELOCITY INCREMENTS IN FT./SEC.

z/C	0.06	0.11	0.16	0.21	0.26	0.38	0.51	1.51	2.51
$q-V$ (*M*) ...	12.1	10.2	8.9	7.5	6.5	5.1	3.7	1.2	0.6
$q-V$ (*F*) ...	12.1	10.0	8.6	7.5	6.5	5.1	4.0	1.2	0.5

Comparing all the results that have been noticed, it appears that very little further work should suffice to build up experimentally a reliable formula for the speed variation above the centre of span of aerofoils. Assuming the form of (3) to be sufficient, A would evidently best be a function of the lift coefficient or angle of incidence, while B and C might be arranged to take some account of scale and plan form. The presence of the constant C is probably consistent with the assumption of cyclic flow due to a distribution of vortices along the chord. The testing of Prandtl's theory by means of the first factor of the R.H.S. of (3) is, as will be seen, too difficult at low speeds since the factor only takes effect at considerable distances where the added velocity is small.

In conclusion of this discussion, the speed curves for three positions along the chord of an aerofoil similar to Model C and at the same angle are given in Table IV. The well-established curve over the leading edge and the high value of the maximum velocity measured there will be noticed, but it is to be borne in

mind that these curves do not give the variation along lines which cut the streamlines at right angles.

TABLE IV.

Velocity (ft./sec.) above aerofoil similar to Model C. $\alpha = 4.8°$.

$(q/V)^2$

z/C (above L.E.).	Above L.E.	Above Mid Chord.	Above T.E.
0.1	1.88	1.32	1.05
0.25	1.355	1.265	1.08
0.4	1.235	1.21	1.05
0.55	1.18	1.17	—
0.7	1.16	1.145	1.05
0.95	1.095	1.11	1.07
1.2	1.07	1.085	1.065
1.7	1.05	1.06	1.045
2.2	1.032	1.048	1.035
2.7	1.030	1.040	1.028

TABLE V.

Traces of Pitot Boundary on Symmetrical Plane.

Model A (1½in. chord) and Model D (3in. chord). $V = 40$ ft./sec.

(See notes to Table VI.)

Aerofoil.	Angle =		6.8°				11.8°			
			Distance behind L.E. in chords.							
	y/C		1.4		2.07		1.4		2.07	
			z	z^1	z	z^1	z	z^1	z	z^1
A	0		5.8	0.1	6.1	1.5	17.5	3.5	18.3	7.2
			0.05		0.3		0.6		1.0	
D	0		5.7	1.5	4.5	4.5	2.8	6.9	1.5	10.3

On the Form of the Pitot Boundary Behind the Trailing Edge

Going now beyond the trailing edge, we may notice from Fig. 2 the distance apart of the traces of the pitot boundary on the symmetrical plane for the three aerofoils *A*, *C* and *D* at a number of angles. The depth of the "bag"—or the thickness of the specially viscous layer, if we prefer to call it so—increases with angle and distance aft. The actual measurements for these sketches may be found in Tables V., VI. and VII. Model A stalled at a somewhat greater angle

TABLE VI.

SECTIONS THROUGH PITOT BOUNDARY, MODEL C (21IN. CHORD, 111N. SPAN).

(V = 40 ft./sec.)

z = distance of upper trace above local level of trailing edge in m/m.

z' = ,, lower ,, below ,, ,, ,, ,,

a means distance less than about 0.3 m/m., — means no measurement attempted.

Angle =	2.8°		7.2°									11.9°						
	Distance behind L.E. (in chords).																	
y C from centre-line of span.	0.75	1.3	0.25	0.75	1.3		1.75		3.25			0.25	0.75	1.3	1.75		3.25	
	z	z	z'	z	z	z'	z	z'	z	z'		z	z	z	z	z'	z	z'
0	1.0	0	3.7	1.7	3.5	5.3	1.7	2.9	2.9	— 2.4	13.0	3.3	14.1	22.8	27.4	6.0	36.8	10.2
0.25	1.3	— .4	3.9	—	5.6	4.5	1.2	3.1	2.1	— 0.9	11.5	2.5	11.5	21.0	23.8	6.0	34.3	12.5
0.5	1.6	— .6	2.6	2.6	4.3	3.3	— 0.3	3.5	1.3	— 2.7	10.2	3.5	9.4	14.6	17.8	6.5	17.0	12.6
0.75	1.5	— .2	3.2	—	6.2	5.3	1.2	4.3	1.7	— 1.0	10.7	3.3	8.1	7.8	9.8	5.2	6.6	9.0
1.0	1.3	.3	4.1	3.5	5.6	4.5	1.6	3.3	1.8	— 3.3	11.5	2.8	5.8	6.0	6.8	7.2	3.3	13.4
1.5	0.3	.9	3.3	a	3.1	2.8	0.1	2.1	0.7	— 2.7	7.9	3.3	3.3	1.8	0.8	5.8	— 4.5	10.7
2.0	a	1.3	0.2	a	5.8	4.3	0.1	2.0	0.2	— 2.2	6.9	1.8	3.8	3.3	2.2	4.4	— 0.4	5.9
2.25	a	1.4	— 1.0	—	4.0	5.3	0.2	4.8	0.2	1.6	3.7	—	—	—	—	—	18.6	3.5
2.5	a	2.5	— 2.4	a	2.5	5.9	— 1.3	8.0	— 0.5	10.1	0.0	a	0.6	9.5	12.3	— 0.1	19.3	— 2.1
2.75	—	a	a	—	a	a	a	a	a	shallow		—	—	—	8.0	— 4.7	15.3	6.1
3.0	—	—	—	—	—	—	—	—	—	—	—	a	a	a	a	a	—	—

than that at which it was set for the two readings in front of the trailing edge, but the presence of the pitot tubes may have hastened the critical angle. Looking at the curves, however, it may be estimated that the pitot boundary will recede, say, 0.25 *C* from the trailing edge before the critical angle is reached. Model C stalled at between 10deg. and 11deg.; the lift curve was afterwards very flat, the lift at 12deg. being 0.94 of the maximum. It will be seen that the specially viscous layer can only be regarded as very thin over the under surface and the front part of the upper surface.

Measurements for outside the symmetrical plane with Model C are included in Table VI., and some of these are plotted in Fig. 3. Downwash experiments showed that this model was devoid of cored vortices at flying angles, and there is an interest in comparing its boundary at 7.2deg. with that of an aerofoil where vortices were present. This may be done by inspecting the lower part of the figure, where a section through the viscous layer of the rectangular aerofoil of R.A.F.15 section at 4.8deg. is shown (Table VII.) recording the measurements. The circular region near the wing tip of the latter aerofoil encloses very marked pitot loss; the region is nearly isolated from the rest of the layer, for the link connecting the two is a shallow depression. The position of the centre of the vortex core with the circular region is marked with a cross. This feature is absent from the former model, and 10 per cent. or so of drag is thereby directly saved. The substantially thinner viscous layer behind *C*, except at the centre of span, will also be noticed.

TABLE VII.

SECTION THROUGH PITOT BOUNDARY OF MODEL D AT 4.8°, 2C BEHIND T.E.

(3in. chord, $V = 40$ ft./sec.)

(See notes to Table VI.)

$y/C =$	0.0	1.0	2.0	2.5	2.66	2.86	2.9	3.0	3.15
z =	0.0	—1.5	—3.0	—7.6	—8.3	+ 16.0 / — 7.6	+ 19.0 / — 6.5	+13.0	—
z^1 =	15.3	16.5	16.8	21.3	19.1	2.3 / 15.2	1.5 / 14.0	6.5	—

The change of the boundary of *C* behind the extremity of span caused by the stall shows the beginning of the development of a vortex. The layer has also thickened, though not unduly so, behind the shoulder of the wing tip, but the effect increases rapidly towards the centre of span and may be associated with the early loss of lift there.

I conclude by repeating that such measurements as the present make a case for the remark that viscosity as such is of fundamental importance in determining the system of flow.

I have great pleasure in mentioning that most of the readings to which reference has been made were obtained initially for the Commercial Aeroplane Wing Syndicate, and in acknowledging their permission for publication. The Department of Scientific and Industrial Research provided for the completion of the work.

THEORETICAL RELATIONSHIPS FOR THE LIFT AND DRAG OF AN AEROFOIL STRUCTURE

BY H. GLAUERT, M.A., A.F.R.AE.S.

1 *Introduction*

It is a fact of common experience that a body in motion relative to a gas or liquid is subject to a resultant force, and it is customary to resolve this force into two components, the drag opposing the relative motion and the lift at right-angles to the direction of this motion. In general the drag is the predominant component, but the class of bodies known as aerofoils and used for the construction of aeroplane wings, is such that the lift is considerably in excess of the drag. The present discussion relates solely to this class of bodies whose essential characteristic is the production of a large lift correlated with a relatively small drag. It is a matter of very considerable importance to develop a theory which will explain the origin of the forces experienced by an aerofoil, and will provide a method of calculating the characteristics of any aerofoil structure from a knowledge of its geometrical form and of the physical properties of the fluid through which it moves. It is proposed to discuss the behaviour of aerofoils in two and in three dimensions, and in particular to discuss the vortex theory of lift and induced drag. The general nature of the flow pattern on which the theory is based has been described by Lanchester, but the mathematical development of the theory is due to Prandtl and his colleagues.

The solution of a physical problem can be analysed into three parts, each of which involves its own special difficulties. In the first place it is necessary to formulate the physical assumptions as to the nature of the bodies and fluid involved in the problem. It is necessary to come to a decision as to the legitimacy of neglecting gravitational and other effects, of regarding the bodies as rigid and the fluid as incompressible and non-viscous. It is only on the rarest occasion that it is deemed necessary to retain the full complexity of the physical nature of the bodies and fluid, but the value of the solution obtained will always depend on the validity of the initial physical assumptions. The second part of the solution consists of the mathematical statement of the problem, usually in the form of differential equations. This statement will be a full and accurate representation of the physical assumptions and will comprise both the equations of motion and the initial and boundary conditions. In a certain sense the problem can now be regarded as solved, since the third step consists only of the mathematical manipulation of the equations into a form more suitable for deducing numerical results which may be compared with experience or used in design. This third step, however, often presents almost insuperable difficulties, and it may become necessary to confine our attention to certain special types of problem, to restrict the motion to small deviations from a known motion, or to simplify the original physical assumptions by neglecting those terms which the mathematical analysis indicates to be of small importance. The value of the final solution decreases with the simplification of the physical assumptions, but it is important to remember that in no case are these assumptions absolutely rigid, and that in all cases the extent of justifiable approximation depends on the accuracy of the results deduced when compared with experience.

2 *Aerodynamic Problems*

Progress in solving aerodynamic problems has been delayed by the difficulty of formulating simple but accurate assumptions as to the nature of the fluid. The

old corpuscular theory was a failure and the kinetic theory of gases is too complex. Classical hydrodynamics assumes a continuous compressible viscous medium, but owing to the difficulties of mathematical treatment, the majority of the solutions refer to a perfect fluid which possesses neither viscosity nor compressibility. In addition the motion is usually assumed to be irrotational, by reason of the established theorem that rotation or vorticity can be neither created nor destroyed by a conservative system of forces.

The solutions for the steady irrotational motion of a perfect fluid fall into three classes. In the first place solutions have been obtained for the steady acyclic flow past different bodies, but it was at once evident that the initial assumptions had destroyed all reality in the final solutions. The analysis showed that the body would experience no resultant force, and in consequence the theory failed to account for the observed phenomena. An attempt to overcome this difficulty was made on the lines suggested by Helmholtz, involving the existence of surfaces of discontinuity of velocity springing from the surface of the body. In this way a resultant force is obtained on the body, but the agreement with experimental values is poor. Further, it has been shown that these surfaces of discontinuity are unstable and break up into vortices. This method, however, does suggest an analogy with the actual behaviour of bluff bodies which shed a series of vortices. The third type of solution is that of cyclic flow, when there is a circulation round the body. The theory of a perfect fluid suggests no method by which this circulation might arise, but if the circulation is present the analysis leads to the conclusion that in two-dimensional motion there is a resultant force acting on the body at right angles to the direction of motion, and of magnitude

$$L = \rho V K l$$

where ρ is the density of the fluid, V the relative velocity, K the circulation, and l the length of the body. Apart from the criticisms already mentioned, all these solutions suffer from the defect that the fluid has a finite tangential velocity at the surface of the body, whereas experiments have shown that in all cases the fluid immediately adjacent to the surface has no velocity relative to the body. A further criticism can be made in the case of bodies which have sharp edges or regions of small radius of curvature. At such a point the theories indicate excessively high velocities and very low pressures. On account of the low pressure the fluid can no longer be regarded as incompressible, and on account of the high velocity gradient viscous forces become relatively more important, and it is certain that even if the theory were found to be satisfactory in general, it would require modification near these particular regions.

In view of this discussion it appears that no satisfactory solution of an aerodynamic problem is to be expected when the effects of compressibility and viscosity are neglected, and it becomes necessary to consider the effect of these two factors. Assuming the general mass of the fluid to be at rest, compressibility only becomes of importance when the velocity of the fluid in any small region approaches to the velocity of sound. In the case of projectiles or high-speed airscrews, the velocity of the body is such that compressibility effects are of prime importance, but it is proposed to limit the present discussion to moderate velocities for which the compressibility of the fluid can be ignored. There remains, however, the possibility that the velocity of the fluid near a sharp edge of the body may reach so high a value as to introduce compressibility effects.

Turning next to the question of viscosity, there is the experimental fact that there is no slip at the surface of the body. Also as the viscous force is proportional to the rate of change of velocity, it is evident that the viscosity will be of importance in the immediate neighbourhood of the body, but may be negligible at large distances from it. The equations for viscous flow are known, but present considerable difficulties of solution, and the question arises to what extent approxi-

mation to these equations is legitimate. It is known that the solution obtained by ignoring the viscosity is unsatisfactory, but it is by no means obvious that the limiting solution obtained as the viscosity tends to zero is the same as the solution for zero viscosity. In particular, in the case of a body with a sharp edge, there is a region where the velocity gradient tends to infinity, and where the viscous forces will be of the same order as the dynamic forces, however small the viscosity. On the other hand, the layer round the body in which viscosity is of importance can be conceived as of zero thickness in the limit, and this conception is equivalent to allowing slip on the surface of the body. It appears, therefore, that the non-viscous equations will be the same as the limit of the viscous equations, except in the region of sharp edges.

One more general point is worthy of attention. The solutions considered above have all been assumed to be steady motions, but it is by no means certain that the solution of any aerodynamic problem is ever a steady motion. On general grounds it seems certain that a steady motion solution always exists in theory, but this solution will correspond to actual conditions only if it be a stable motion. In the case of the flow past a cylinder, the flow is known to be periodic, and the same is true for the flow past an aerofoil at large angles of incidence. It is possible that the flow past an aerofoil is always periodic, but that at small angles of incidence the amplitude of the oscillations may be negligibly small. As a further example the case of the flow along a flat plate may be mentioned, where the steady viscous flow is known but applies only to very small velocities, the flow becoming periodic for higher velocities. Even when a steady flow is obtained, it appears that the possibility of periodic flow may exert an important influence in deciding which of several possible steady motions will actually occur.

3 *Cyclic Flow*

Prandtl's aerofoil theory is based on the belief that a cyclic motion is essential for the production of the lift of an aerofoil, and it is necessary, therefore, to examine the evidence for the existence of this circulation, the method of its origin and the relationship of the solution to the complete equations of viscous flow.

If an aerofoil experiences a lift force the average pressure above the aerofoil must be lower than that below it, and if we consider points at some distance from the wing where the effects of viscosity are negligible, this implies that the average velocity is higher above the aerofoil than below it, and so indicates a circulation round the wing. Alternatively by considering the pressure distribution across the span of a wing, we can see that the streamlines must be converging above the wing and diverging below it, leading to the conclusion that a sheet of velocity discontinuity or vorticity trails behind the wing, and it is evident from fundamental principles that this vortex system must be completed by circulation round the wing. This conclusion is confirmed by all the experimental results which are available. The wing tip vortices have been observed on several occasions, and several comparisons of the flow pattern round aerofoil structures have shown good agreement between the observed flow and that calculated from Prandtl's theory. In fact the association of the lift of an aerofoil with cyclic motion can be regarded as definitely established.

The method in which this cyclic motion originates is somewhat obscure in detail, but the general lines of its development are not difficult to visualise. The irrotational acyclic flow of a perfect fluid past an aerofoil involves the existence of a stagnation point on the upper surface near the trailing edge, and the occurrence of high velocities near this relatively sharp edge. This solution is clearly at fault, but appears to represent the general type of flow at very low velocities. As the motion proceeds a succession of transverse vortices will be shed from the aerofoil, and this process will continue until conditions are reached which no longer give rise to the formation of vortices. In so far as the theory

of a perfect fluid is valid, this implies that the rear stagnation point must move to the immediate neighbourhood of the trailing edge, and this change in its turn implies that a cyclic motion round the aerofoil has developed, the strength of the circulation being equal in magnitude and opposite in sign to the total strength of the transverse vortices which have passed down stream. It is possible that steady conditions are never reached, and that alternate vortices of opposite sign continue to leave the aerofoil, but this would merely imply the oscillation of the circulation round a mean value. The conditions which give rise to the formation of vortices are not well known, but the experimental fact is confirmed by observations of the flow past many different types of body.

Finally we must consider the relationship of Prandtl's analysis to the complete equations of viscous flow. Prandtl's solution is a first approximation to the accurate equations, and as such it is legitimate to neglect the effects of viscosity except in those regions where the velocity gradient is large, *i.e.*, near the surface of the aerofoil or in the cores of any vortices which may exist. In other regions it is legitimate to use the equations of a perfect fluid. This method receives confirmation from G. I. Taylor's experiments with rotating fluids. The non-viscous solution for the case of a sphere moving along the axis of rotation of a fluid shows no slip at the boundary, and in this case the non-viscous solution agrees well with experiments. Since to the first order the vortices in the fluid can be regarded as line vortices, the region where viscosity retains its importance is confined to a narrow sheath round the aerofoil. Thus the approximations used by Prandtl are equivalent to the use of the equations of a perfect fluid, modified by the condition that vortices may arise at points of the body where the streams passing above and below the body re-unite. The only doubtful point of this approximation appears to be in the assumption that the vortex wake behind the aerofoil can be regarded as of zero thickness, and it is on account of this limitation that the theory can only be applied to small angles of incidence, and gives no account of the stalling of the aerofoil at its critical angle.

The real test of the validity of Prandtl's approximations lies in the comparison of the results deduced from this theory with actual experimental results, and on this basis the approximations appear to be fully justified. The theory has now been tested in a variety of ways, by examination of the flow past aerofoil structures, by comparison of the effect of aspect ratio and of the effect of complex multiplane structure, and in almost every case the agreement between theory and experiment has been far better than would be expected from the nature of the approximations. The application of the theory is, however, limited to aerofoils of good shape at small angles of incidence, and excludes the region in the immediate proximity of the surface of the aerofoil. These limitations, however, which are essential to the nature of the approximations made, do not detract in any way from the validity of the solution in the region to which it applies. It must also be emphasised that Prandtl's theory is based on the association of the lift of an aerofoil with cyclic motion, but does not give any indication of the relationship between the magnitude of this circulation and the geometrical form of an aerofoil. This problem, which has been attacked by Kutta, Joukowski and other writers, is quite distinct from the work of Prandtl, and the validity of Prandtl's theory does not in any way depend on the validity of Joukowski's hypotheses.

4 *The Two-Dimensional Theory*

The flow round an aerofoil in two dimensions is represented by the equations of a perfect fluid on the assumption that the vortex layer surrounding the aerofoil is of evanescent thickness. The assumption will clearly break down if the solution involves excessively large velocity at any point, and in all cases it can only be regarded as an approximation to the actual flow. The only difficulty in

obtaining a solution on these lines is the uncertainty of the value which shall be assigned to the circulation, and no solution of the general case has been obtained. Joukowski has, however, suggested the hypothesis that for the class of aerofoils which have a sharp trailing edge, the circulation will be such that the rear stagnation point is situated on the trailing edge. The justification of this hypothesis is that the solution is consistent with the assumption of a vortex layer of evanescent thickness, while any other position of the stagnation point would violate this assumption. The hypothesis can only be applied to a limited class of aerofoils, but the results obtained are very hopeful. In particular the slope of the lift curve is found to be slightly larger than π per radian in all cases, and this agrees excellently with values deduced from experimental results by applying Prandtl's correction for aspect ratio.

It is not proposed to enter into the method of solution or its many applications in any detail, as the subject is a large one, worthy of separate attention. The method is based on the known cyclic flow round a circle, and by means of a suitable conformal transformation the flow round an aerofoil is obtained, the value of the circulation being chosen in conformity with Joukowski's hypothesis.

The Joukowski series of aerofoils is obtained by the conformal transformation

$$Z_1 = Z + c^2/Z$$

which transforms a circle in the Z plane to an aerofoil in the Z_1 plane. The origin of the circle is arbitrary, but the circle must pass through the point $Z = -c$ and enclose the point $Z = c$. There is therefore a doubly infinite series of Joukowski aerofoils which all have the common characteristic of a cusp at the trailing edge.

An extension of this method is obtained by use of the transformation

$$(Z_1 - nc)/(Z_1 + nc) = (Z - c)^n/(Z + c)^n$$

applied to the same set of circles as the previous transformation. This leads to a class of aerofoils which have a sharp trailing edge, but whose surfaces at this point meet at an angle $(2 - n)\pi$. Thus an aerofoil of conventional type is obtained by choosing n slightly less than 2.

The most general transformation can be written in the form

$$Z_1 = Z + a_1/Z + a_2/Z_2 + \ldots .$$

where the coefficients a_1 a_2 . . . may be complex. The transformation is applied to a circle which has one of the zeroes of dZ_1/dZ on its boundary and encloses the other zeroes. If a be the radius of the circle and if $a_1 = c^2\, e^{2i\gamma}$, it can be shown that the lift of the aerofoil is

$$L = 4\pi a \rho V^2 \sin(\alpha + \beta)$$

and the moment round the centre of the circle

$$M = 2\pi c^2 \rho V^2 \sin 2(\alpha + \gamma)$$

where α is the angle of incidence and β is the angle between the x axis and the line joining the centre of the circle to the zero of dZ_1/dZ on its boundary.

5 *The Three-Dimensional Problem*

Prandtl has developed the theory of an aerofoil in three dimensions on the lines of the flow patterns suggested by Lanchester and on the basis of a definite association between lift and circulation. The analysis, however, does not claim to establish from first principles the relationship between circulation and geometrical form, but provides a method of deducing the characteristics of one aerofoil structure from those of another structure with the same aerofoil section. The theory is, therefore, quite independent of the accuracy of Joukowski's hypothesis, and is not limited to the class of aerofoils with sharp trailing edges.

In general the solution is a first order approximation only, but there is no reason why the solution should not be carried to a higher order of accuracy at the expense of greater complexity, if this course should be considered necessary or desirable. The excellence of the results obtaind by the first order solution is the justification of the approximations made.

The lift of an aerofoil is associated with a definite circulation, and to complete the system, vortices spring from the trailing edge. To the first order these may be regarded as line vortices in the direction of the general stream. A consideration of the velocity field of the vortex system shows that there is a normal velocity in the region of the aerofoil by virtue of which the effective angle of incidence is reduced and the resultant force has a drag component, to which the name of induced drag has been given. The mathematical statement of these effects presents no difficulties, but the detailed analysis is rather complex except in a few special cases.

It has been shown that the minimum induced drag is obtained when the normal induced velocity due to the trailing vortex system has the same value at all the lifting elements. In the case of a single monoplane aerofoil this will occur when the lift is distributed elliptically across the span of the wing. The formulæ obtained then are

$$\alpha = \alpha_0 + Sk_L/2\pi s^2$$
$$k_D = k_{D0} + Sk_L^2/2\pi s^2.$$

In these equations α, k_L, k_D are the charasteristics of the aerofoil of area S and semispan s, while α_0, k_L, k_{D0} are the characteristics of the same aerofoil section in two-dimensional flow. The drag represented by k_{D0} is known as the profile drag, and is due entirely to viscous forces.

These formulæ refer to the condition of minimum induced drag, but also give quite good approximations to all conventional shapes of aerofoil. The case of a rectangular wing has been worked out in detail, and it has been found that the induced drag is about 5 per cent. higher than the value indicated by this minimum formula.

In the case of a multiplane structure, an aerofoil element is subject to induced velocity due to its own trailing vortices, and also to the vortex systems of the other aerofoils. The analysis becomes increasingly difficult, but has been fully justified by comparison with experiments, even in the case of a multiplane structure consisting of five aerofoils at a large negative angle of stagger. The analysis of a multiplane structure is simplified by an interesting theorem as to the effect of stagger. The induced drag of the system can be measured by the kinetic energy of the field of the vortex system far behind the aerofoils, and so it appears that the induced drag will not be affected by moving an aerofoil element in the direction of motion, provided the incidence is adjusted to maintain the same lift on all the aerofoil elements. By this means a staggered structure can be replaced by an equivalent unstaggered structure as regards total induced drag.

6 *Conclusion*

The object of this Paper has been to attempt a discussion of the fundamental principles of the modern aerofoil theory, rather than to give a detailed account of its varied applications or of the many tests which have been applied to test its accuracy. References are, however, given below to the principal reports which deal with these other aspects of the aerofoil problem.

The two-dimensional problem provides an interesting field of work, involving conformal transformation, but has scarcely reached the point of direct practical utility as yet. Further work is required to examine the question of the viscous drag of an aerofoil, and the method of conformal transformation must be developed so as to be capable of dealing with the aerofoil sections actually in use.

The three-dimensional analysis has already provided results of considerable value, and has been repeatedly confirmed by comparison with experiments. It provides a satisfactory method of predicting the characteristics of an aerofoil structure, however complex, from the known characteristics of a monoplane aerofoil of the same section. Further interesting applications have been the interference experienced by an aerofoil tested in a wind channel due to the constraint of the walls, and the development of an airscrew theory on lines suggested by the vortex theory of aerofoils.

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CORRESPONDENCE

Audit House,
Victoria Embankment, E.C.4,
17*th July*, 1923.

To the Editor of the JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY.

SIR,—Professor Orr, of the Royal College of Science, Dublin, has been kind enough to send me some criticism in connection with my paper, "On the Stability of Aero Engines," which appeared in the Journal for April, 1923.

I do not propose to deal yet with the more general criticism, but as a sequel to Professor Orr's remarks the formulæ given in my paper abbreviates to a remarkable degree and the elegant results are free from the reproach of being "complicated mathematical formulæ."

As Professor Orr naively writes, "What labour you might have saved yourself!"

Yours faithfully,

J. MORRIS.

CASE I.

Direct Drive

All throws and intermediate journals equal (Fig. 1).

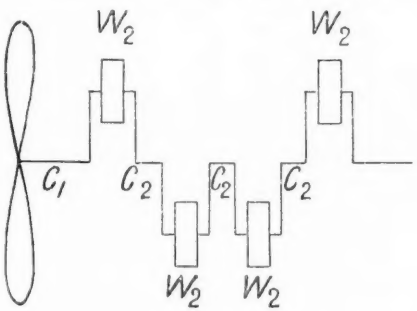

FIG. 1.

Let

I_o = moment of inertia of one throw (with crankpin but without rotating load) about the crankshaft axis.
W_2 = rotating load at crankpin.
l_2 = the length of a throw from crankshaft centre to crankpin centre.
c_2 = torsional stiffness of a journal (*i.e.*, its CI/L where C is in lbs./in.^{2}. I in ins.^{4} and L in ins.).

The polar moment of inertia of the airscrews is considered to be very large.

Then the frequency of *one throw complete* about a *single journal* is

$$f_2 = (1/2\pi)\sqrt{(c_2/p_2)} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where

$$p_2 = (I_o + W_2 l_2^2)/g = (W_o + W_2)\, l_2^2/g \text{ (say)}$$

g being in ins./sec.^{2}.

Let c_1 = torsional stiffness of the shaft between the airscrew and the first throw and let

$$c_1 = rc_2$$

Then the frequencies of the system in torsional oscillation are given by the formula

$$f = 2 f_e \sin \alpha/2 \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where α is given by

$$\tan \tfrac{1}{2}\alpha \tan n\alpha = r/(2 - r) \quad . \quad . \quad . \quad . \quad (3)$$

n being the number of throws.

Example

Take a four-throw crankshaft driving an airscrew direct. Let the dimensions of the journal be $1\frac{7}{8}'' \times 1''$ (dia.) and those of the shaft between the airscrew and the first throw $6'' \times 2''$ (dia.).

We find

$$c_2 = 10^6 \pi/5$$

and

$$c_1 = \pi \times 10^6$$
$$c_1 = 5 c_2$$

By formula (3)

$$\tan \tfrac{1}{2}\alpha \tan 4\alpha = -5/3$$

From the tables or otherwise we find that the lowest value of $\alpha/2$ to satisfy is

$$\alpha/2 = 12^\circ 10' \text{ approx.}$$
$$(\tan 12^\circ 10' \tan 97^\circ 20' = -1.68).$$

So that by formula (2)

$$f = 2 f_2 \sin 12^\circ 10'$$
$$= .422 f_2$$

Let now

$$W_0 + W_2 = 8 \text{ lbs.}$$

and

$$l_2 = 3 \text{ ins.}$$

Then

$$p_2 = 8 \times 9/384 = 3/16$$

Hence the fundamental frequency of the system is

$$(.422/2\pi)\sqrt{(\pi \times 10^6 \times 16)/15}$$

or approximately 122 double vibrations per second.

(*Cf.* this with the solution of the same problem on page 213 in the JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY for April, 1923.)

CASE II.

Geared Drive (Fig. 2)

Otherwise as in Case I. The inertia of the gear wheels is supposed small.

Let ρ_1 be the gear ratio (R_1/r_1).

Then if we write

$$1/c_1 + \rho_1^2/c_1^1 = 1/C_1$$

and

$$C^1 = rc_2$$

the subsequent procedure is as in Case I.

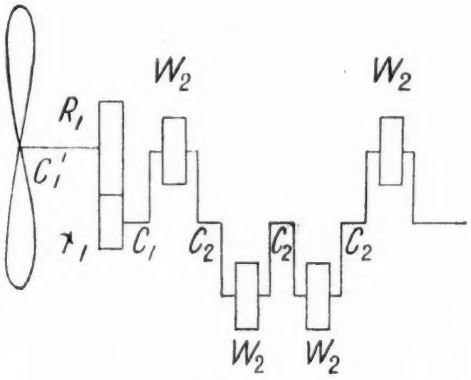

FIG. 2.

CASE III.

Double Reduction Drive (Fig. 3)

Otherwise as in Case I.

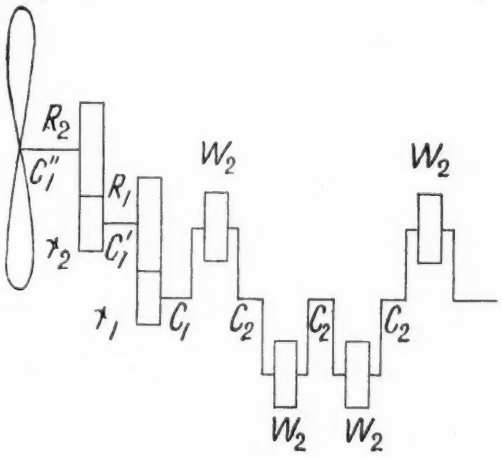

FIG. 3.

ρ_1 = first gear ratio (R_1/r_1)
ρ_2 = second ditto (R_2/r_2)

Then if we write

$$1/c_1 + \rho_1^2/c_1^1 + \rho_1^2\rho_2^2/c_1'' = 1/C_1$$

and

$$C_1 = rc_2$$

the subsequent procedure is as in Case I.

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